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———. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29: 345–66.

Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32: 5–19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

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Literary Culture in France and the United States[1]

Priscilla P. Clark
University of Illinois at Chicago Circle

A series of cultural indicators reveals the similarity of general population literary preferences and the dissimilarity of elite literary tastes in France and the United States. The distinctive associations of literary activities with French and American elites are responsible for the distinctive characters of French and American literary cultures. Whereas French literary culture is sustained by strong ties to centrally located governmental and traditional elites, American literary culture is defined by its support from dispersed private elites. As these ties to elites define literary culture and influence literature, they are supported by the social and political organization of French and American society. Recent social and political changes have modified traditional patterns of support for literature and have attenuated but not effaced certain distinctive features of French and American literary cultures.

> The relations that exist between the social and political conditions of a people and the genius of its authors are always numerous; whoever knows the one is never completely ignorant of the other. [DE TOCQUEVILLE 1958, p. 63]

I

Associated with elites in premodern societies, the arts in modern times find themselves in a world often indifferent, and at times frankly hostile, to anything labeled "elitist." The resulting tension between the traditional elite orientation of the arts and a certain populism in contemporary mass societies is fundamental to the dynamics of art today. For although one can argue that never as at present have the arts so flourished, concerned so many, or received so much support, because they cannot depend on market support, by necessity as much as by inclination the arts seek out elites today as in the past. Perhaps more important still, beyond the crucial economic support they provide, the elite ties of the arts sustain artistic cultures—

[1] It is a pleasure to acknowledge assistance, both intellectual and material: research grants from the University of Illinois at Chicago Circle; research assistance from Michelle Fox, Elizabeth Gross, Lee Ann Nicholson, Greg Pittsley, Michael Ralston, George Rezek, Judy Wattelez, and Lucie Yassa; interviews and information from French officials in Chicago, New York, and Paris; and criticisms and comments from too many quarters to be enumerated lest I be forced to declare intellectual bankruptcy.

those values, norms, symbols, and patterns of behavior that create a distinctive milieu for the practice, pursuit, and appreciation of a given art.

I shall, however, argue the case only for literature and literary culture, and only for France and the United States. In very large measure the distinctive literary cultures of these countries derive from the very different elite associations of literature in each country. With roots in an aristocratic, monarchical, and highly centralized society, French literary culture is characterized by long and close associations with those traditional elites, notably governmental, which have been central to and especially prominent in French society; this in marked contrast to American literary culture, which evolved in a society that explicitly rejected aristocratic models of society and literature. The elites associated with American literature have been located in the private rather than the public sector and dispersed over the country rather than concentrated in the capital. The two literary cultures could not but differ profoundly as indeed they still do, despite recent modifications of established patterns of elite interaction which have attenuated though not effaced their once very great differences.

To explore these literary cultures I shall make use of what may be termed cultural indicators. Whereas economic indicators generally gauge the performance of the economy and social indicators typically point to characteristics of populations, cultural indicators aim at defining cultural attitudes, ideals, and dispositions. As social and economic indicators often coincide, so cultural indicators will frequently intersect with each of them. Thus whether given data are used to construct an economic, social, or cultural indicator will depend on what is wanted from the analysis.[2]

II

The distinctive literary tastes of French and American elites are all the more striking in view of the quite similar literary preferences of the general French and American populations. Several indicators point to this similarity: the number of titles published annually, sales of books, reading habits, status of writers, and total moneys in direct support available to writers.

Although France publishes more new titles annually (52.2 per 100,000 inhabitants in 1973) than the United States (39.7 titles), on a world scale

[2] George Gerbner and his associates have used the term quite differently in their analyses of the composition, structure, and effects of the "messages" of television dramatic programs, principally with regard to the incidence of violence. In this usage cultural indicators refer both to the "cultural" content of television programs and also to the broader sociocultural views reinforced by this content (see Gerbner 1973 and Gerbner et al. 1978). "Cultural indicators," as I use the term, are concerned neither with media content nor with its effects but are used to define the institutions, behavior, values, and symbols of a particular milieu or (sub)culture, in this case the ambient culture of literary activities.

both countries are in the same upper-middle range.[3] And if, of all books published, France continues to publish a higher percentage of literary titles, again on a world scale the United States does not lag that far behind nor is France by any means the leader. The proportion of literary titles has declined in both countries: from 45% of American publications in 1811–20, literature decreased to 42% of all titles in 1900, 35.9% in 1937–39, 37% in 1950, 35.9% in 1960, and 22.3% in 1973; France published 28.3% literary titles in 1938, 31.5% in 1950, 35% in 1960, and 26.2% in 1973.[4] As in other industrialized countries the increase in scientific and technical titles has counterbalanced the relative decrease of literary works.

In neither country are books a major commodity: in France and in the United States, sales of books represent a tiny and virtually identical proportion of the GNP (0.28% in 1970 for the U.S., 0.26% for France).[5] And since the French on the whole seem to read about as much (or as little) as Americans,[6] we should probably not be startled at the virtually identical scores obtained by writers in France and the United States on the NORC Occupational Prestige scale.[7] Finally, relative to their populations, France

[3] Based on 1973 totals from the *United Nations Statistical Yearbook* (United Nations 1976) (French totals include pamphlets). Titles per 100,000 inhabitants ranged from 0.9 to over 100. Over half (51) of the 81 reporting countries publish fewer than 20 titles per 100,000 inhabitants. This and the other cultural indicators are discussed in greater detail in Clark (1979).

[4] The 19th-century average is reported in Charvat (1968, p. 35); the 1900 figure is from Hokkanen (1969, p. 67); later American and all French figures are taken from the *United Nations Statistical Yearbook* (United Nations 1954, 1963, 1973, 1976). The United Nations "Literature" category (number 8, Universal Decimal System) partially covers the following categories of the American Publishers Association: "Fiction," "Juvenile," "Literature," "Poetry, drama." Other designations of "literature" or "literary" (prizes, book reviews, television programs, names of streets) represent my own judgment.

[5] Sales in the United States in 1970 were $2,922,000,000 or 0.28% of the GNP. Sales in France in 1970 were Fr 1,935,000,000 before taxes, or $450,000,000 ($1.00 = Fr 4.30), 0.24% of the GNP; Fr 2,069,000,000 or $481,160,000 with value-added tax included, 0.26% of the GNP.

[6] Ennis (1972, p. 51) reviews reader surveys of adult Americans (1949–62), which showed a range of 46%–60% of those surveyed having read a book within the previous year. A 1973 survey in France showed that 69.7% of the sample had read a book within the previous year, but the survey included those in the 15–19 age group, who read more than adults, whereas the American samples were limited to adults. In the most thorough comparative study of reading habits, the French, who averaged seven minutes per day reading books, did not differ greatly from the Americans, the sample of whom spent five minutes on books. Of the French sample 12% read books compared with 7% of the Americans. The differences exist, to be sure, but they are not great, especially in comparison with the 29 minutes a day the Russians spent reading books or the 17 minutes of the runner-up Poles (Szalai 1972, p. 580).

[7] On a scale of 100, "auteur de roman d'avant-garde" ranked 55.7 in France (Clark and Clark 1974) as compared with 57.0 for "author" in the United States, 64.6 in Belgium, 66 in Poland, 70.6 in Israel, and 71.0 in Italy (Treiman 1977). Whatever the arguments against the Occupational Prestige Scale, they presumably apply in one country as another.

and the United States in 1975 provided about the same amount of direct support for writers in fellowships, prizes, and grants (Clark 1976, 1977).

Should we expect otherwise? Both highly industrialized and technologically advanced, France and the United States are also similarly characterized by all but universal literacy, more or less market economies (publishing is not a nationalized industry in France), and more or less representative governments based on universal suffrage. Far greater contrast should be expected between either France or the United States and most so-called Third World or developing countries which are neither technologically advanced nor universally literate nor have representative governments.

The shared literary preferences of the French and American general populations would not provoke much comment were it not for a stereotype which assigns to literature, and to writers, a special and especially important role and/or status in French society—in contrast to the supposed lack of interest and even dislike on the part of the Americans whom Mencken dubbed the "booboisie." Gertrude Stein, for one, was impressed by the deference writers received in France:

> But really what they do do is to respect art and letters, if you are a writer you have privileges, if you are a painter you have privileges and it is pleasant having those privileges. I always remember coming in from the country where I usually kept my car and the garage was full more than full, it was the moment of the automobile salon, but said I what can I do, well said the man in charge I'll see and then he came back and said in a low voice, there is a corner and in this corner I have put the car of Monsieur the academician and next to it I will put yours the others can stay outside and it is quite true even in a garage an academician and a woman of letters takes precedence even of millionaires or politicians, they do, it is quite incredible but they do, the police treat artists and writers respectfully too . . . . [Stein (1940) 1970, p. 21]

One might simply dismiss Stein as out of date and out of step with indicators from the end of the 20th century were it not for more recent statements to similar effect[8] (not counting the number of persons, sociologists included, who have said to me that "of course" the French must read more than Americans, subsidize literature more, think more highly of writers, or whatever). But the contradiction is only apparent. As one might suspect, insofar as in France more attention is accorded things literary or exceptional

[8] Similar sentiments are expressed by nonliterary figures, French no less than foreign. A sampling: a German academic—"France is the only country which has a 'religion' of literature" (Curtius 1932, p. 204); an English journalist—"Often [writers and intellectuals] have been highly vocal rather than directly influential; but always [French] society has treated them as an honored minority, like the holy men of India" (Ardagh 1968, p. 354); an American journalist—It is "a popular belief that the average Frenchman is a cultural *maven*" (Lottman 1975); and most recently a French writer—"It is certain that the status of writer is especially honorable and honored in this country" (Aron 1975, p. 140; cf. pp. 134, 145, 204). Also cf. Wilson (1971, p. 156).

prestige granted literature, such honor and glory derive not from French society as a whole but, rather, from the attitudes and behavior of elites in that society.

And as some indicators point to the congruence of the general French and American populations in regard to literary matters, so others reveal the still considerable differences in the literary preferences of French and American elites. Whereas traditional elites, the monarchy, the aristocracy, the Church, and subsequent governments have especially influenced literary activities in France, American literary life owes more to the untraditional or "modern" elites of commerce and industry.

III

With roots in the princely courts of the Middle Ages and the Renaissance, the elite ties of French literary culture were well established before the 17th century, but it was then that Richelieu and subsequently Louis XIV centralized patronage of literature (as of the arts generally) and associated it with a national government. If all the arts did service, literature was privileged. More clearly than the other arts, it instilled the values and norms of an elite milieu and integrated the individual into that milieu (see Aron 1975, pp. 134, 204). French was a common and exclusive language which not only favored social interaction between *littérateurs* and traditional elites but also affirmed a common elite identity (which is not to say equality). Court and salon alike stressed participation in cultural activities: courtiers and salon habitués not only listened or read or watched, they wrote. Louis XIV himself acted in plays, danced in ballets, and even turned an occasional (poor) sonnet. The genres most in vogue were typically cultivated in salon conviviality—maxims, portraits, epigrams, poetry meant to be read aloud, and plays given private readings. Not withdrawn into the solitary garret of the Romantics, the 17th-century writer belonged in a resolutely social setting in which writing did not define a separate status so much as a role.

While governmental patronage of literature did not disappear altogether after 1789, it diminished drastically (see Clark and Clark 1977; Lough 1978). No longer the usual means of support that it had been during the ancien régime, patronage of individuals became an anachronism, even an anomaly, in the 19th century. One is not really surprised that even the quite fastuous Second Empire should in 1862 budget considerably more money for prizes to horses than for prizes to writers and scholars and allocate more funds to buy horses for the imperial stables than to purchase books for the imperial library. Today the moneys disbursed in direct support to writers are windfalls for a few individuals but have little impact on literary life.

Nevertheless, insignificant as they may be in strictly monetary terms, government subsidies attest to concern for and (presumably) interest in

literature and writers. It is therefore noteworthy that the French government accounts for almost 70% of all direct support to writers in France, while the American federal government supplies only 20% of the direct subventions available in the United States (in 1975; see Clark 1976, 1977). This greater involvement of the national government in France may be largely symbolic, but it is just such symbolic concern that is so striking a feature of French literary culture.

The Académie Française is undoubtedly the best-known because longest-lived embodiment of governmental involvement in the French republic of letters and of the interpenetration of polite and literary society. Founded in 1635, the Académie was charged with fashioning the French language into the proper instrument for French society, which in the 17th century meant the king, the court, and dependent Parisian milieus. It has always drawn its members from many elites (men of letters were and still are a minority) precisely because of this injunction to model the written language on the cultivated speech of salon and court instead of the technical jargon of law or theology (Renan 1860, p. 341). In the same spirit of linguistic punctilio, the Haut Comité de la Langue Française was established in 1967 by Georges Pompidou, then the prime minister, to police the use and abuse of French on national television and radio, while the Académie Française continues periodically to censure incorrect expressions, to purify French of "le franglais" and other contemporary "barbarisms," and to devise authentic French terms as technological and other innovations require(see Bengtsson 1968). This characteristically French "language fetichism" (Bourdieu 1975) or "linguistic imperialism" (Aron 1975, p. 22) recently took Academicians from the tranquillity of the Dictionary Commission to the corridors of the National Assembly where intensive lobbying brought passage of a law which makes it a legal offense and subject to fine to employ foreign or un-French expressions "fabriqués contre le génie de la langue" (as the Académie puts it) in official communiqués: no more "hardware," no more "cockpit," no more "week-end," at least in governmental prose.

The French government is also tied to literary life through a number of literary prizes. The ORTF (Office de la Radio et Télévision Française) awards a literary prize, the Ministry of Education gives two, and not long ago, the Gendarmerie Nationale (National Guard) funded a prize for a novel that would show gendarmes—not ordinary policemen—in action. The Académie Française, the largest single source of prizes, combines private legacies and governmental subsidies to support 143 prizes ($65,000 in 1974), while the Centre National des Lettres recently established seven prizes ($19,000 in 1975).

Striking as they are, French government support and surveillance of letters assume a broader significance as expressions of a more general elite interest in literature. While governmental affiliated groups awarding literary

prizes disbursed 67% of all prize moneys (81% when the Académie Française is included), they represented only 5% of all prize associations (14 of 267) and accounted for not quite 20% of all prizes awarded (160 of 816). With a mean value of $236, literary prizes do not support individuals so much as conceptions of literature, more specifically traditional elite definitions of literature. Although the oldest (and still awarded) literary prize in France dates from 1323 and even though various academies awarded prizes every now and again under the ancien régime, prizes and the associations that awarded them did not begin to multiply until the 19th century—a period when new schools of literature demanding recognition and new publics clamoring for different literary fare were challenging time-honored conceptions of literature. It was then that mass culture first reared its (to most prize donors) ugly head. The bequests received by the Académie Française (the most notorious was for a prize to reward "useful" works) and many of the other prizes funded over the 19th century attest to a conscious defense of traditional values.

In the face of great social as well as literary change, literary prizes offered a guarantee of quality, or more accurately, of a certain, usually traditional, conception of quality. Which is why such a hue and cry is raised whenever the winner of a major prize does not live up to the expectations of publishers as well as readers, for, although the great majority of prizes have no appreciable impact on the market, a few—a very few—do. The Prix Goncourt, awarded since 1902 to the "best work in prose" published during the previous year, regularly sells 200,000–400,000 copies—a regular direct market influence not even approached by any American literary prize. The five or six other "marketable" prizes sell less, but even the 20,000 or so copies of a "difficult" Médicis (established expressly for experimental fiction) are still well above usual sales (only 10% of literary works in France sell over 10,000 copies, 0.5% over 100,000 copies). Nevertheless, and notwithstanding the sales, none of these prizes concerns or is concerned with popular literature. The annual Goncourt is, in fact, denounced annually for its snob appeal, precisely because like most of the other literary prizes it activates essentially elite preferences. The Prix Goncourt simply does so with quite spectacular market effect.

Literary prizes make a specifically literary gesture; newspapers, news magazines, and television address a general public. It is therefore significant that elite interest in language and literature finds expression in general news media. The two leading French newspapers, *Le Monde* and *Le Figaro,* regularly publish columns discussing problems of language usage and reviewing grammars and dictionaries as well as more technical works on language and linguistics. The English (or American) language arouses no comparable general interest in the United States, where such matters tend to be discussed in more specifically literary or "cultural" journals.

This exposure to literature or literary "news" is confirmed by a distribution analysis of *Time* magazine and its closest French counterpart, *L'Express,* which showed that the French weekly consistently allotted more space to cultural concerns generally and to literature in particular. "Cultural" pages accounted for 24% of all pages (less advertising and letters to the editor) in *L'Express* and 16.1% in *Time,* while book and literary pages represented 12% of *L'Express* pages in contrast to 6.9% for *Time.* Since both magazines have a fairly select readership relative to the population as a whole, and since both stress the elite segment of that readership, it seems appropriate to conclude that each reflects an assessment of what will appeal to, hence attract, such an elite readership without alienating the bulk of their readers.[9]

If these media are perceptibly oriented to an elite audience, the same cannot be said of television. Even so, French television programmed more "cultural" material (8% of all hours) than television in the United States (2.6% of all hours in New York City) as well as more literature and drama (3% of Paris hours vs. 1.2% in New York) (*Le Figaro* 1976; *New York City TV Guide* 1976). Still, although the French programs are more prominent because the choice of programs is so restricted, the match is probably a draw in view of the greater total literary and cultural hours on American television.[10] In any event, unlike American television stations whose decentralized ownership orients them to advertisers, French television stations are

[9] Five issues of *Time* magazine and *L'Express* were examined for 1973–74 and five for 1977–78. From 1973–74 to 1977–78 cultural pages remained virtually constant in both magazines, but literary-book pages declined by almost one-third for *Time* (8.4% of all pages to 6.9%) and by close to one-half for *L'Express* (16.1% to 8.3%). The 1975 circulation (paid copies) of *Time* would reach 2% of the American population; the copies printed of *L'Express* would be read by 1.3% of the French population. Even though one should assume several readers per copy (*L'Express* counts on 4.5), the journals' managements stress their elite readership: 35.5% of *Time* subscribers are managers or administrators, 27.2% have master's or doctor's degrees ("Time" *National Subscribers Study* 1976), while 26.3% of *L'Express* readers had upper level management or administrative positions ("cadres supérieurs") (*L'Express* 1974). The greater resonance of books and literature for French elites (and/or the more pronounced elite character of *L'Express*) can be gauged by advertisements: in 13 issues examined for 1978 *L'Express* had over three times as many book or book-related advertisements as did *Time* (53 vs. 16). Moreover, almost half the *Time* advertisements were for *Time-Life* books or magazines and not remotely literary, whereas 40 of the *L'Express* advertisements, or 75%, were literary (mostly novels). One might note also the book advertisements in *Le Monde* and *Le Figaro,* which appear on the front pages as well as in the weekly book section, a practice not followed by American newspapers.

[10] The statistics for New York include public broadcasting. During the week of June 12–18, 1976, New York broadcast 1,750 hours total, including 45 hours (2.6%) of "cultural" programs, 22 hours (1.2%) of literary and theater programs, and 6 hours (0.3%) of literary programs alone (*New York City TV Guide* 1976). Paris television in the week of April 12–18, 1976 totaled only 189 hours, of which 15 (8%) were "cultural" programs, 5.75 (3%) literary and theater programs, and 1.83 hours (0.97%) literary programming (*Le Figaro* 1976).

administered by government appointed officials at least as mindful of what is appropriate as of what is popular. The relative prominence of "cultural" programs on French television undoubtedly reflects a judgment that these are eminently appropriate subjects. It is obvious, however, that whatever the prominence of individual programs or the publicity given to "cultural" specials on American networks, the programs themselves are at best an oasis of culture. Even in France the officials who program television broadcasting are dealing with a mass medium.

Not so the government officials who select a country's bank notes, who are relatively free to select the images they feel most appropriate for this "cultural carrier." And this indicator is striking evidence of how very appropriate a cultural carrier literature is in France. In contrast to other European countries as well as the United States, literature does spectacularly well on French bank notes: writers account for five of the seven figures on bills currently in circulation (Hugo, Voltaire, Racine, Corneille, Pascal). The individual's portrait is on both sides of the note, against a background related to incidents or places important in his life. In contrast, all American bills show political figures, the Federal Republic of Germany uses portraits from famous German paintings, and Great Britain and Italy each claim one writer (Shakespeare, Manzoni) out of five figures.[11] Moreover, the French bank notes do not identify the writers, unlike the Italian, British, and American notes and unlike the most recent French issues with a composer (Berlioz) and a (minor) painter (Maurice Quentin de la Tour). Presumably the writers were considered recognizable, although an unrepresentative personal sample was highly inconclusive in this regard. Still, it is not entirely improbable that prostitutes should pay off cops with "des Pascal" (street slang for 500-franc notes), however unlikely it is that either party to the transaction has read a word by the 17th-century writer, geometrician, and mystic.

City officials too select cultural symbols—mottos, emblems, flags, and names of streets. Paris City Councils have concurred in their city's reputation as a literary center, for 332 streets in Paris, 6.4% of all streets, bear

[11] For comparison, U.S. bills portray: \$1.00—Washington, \$2.00—Jefferson, \$5.00—Lincoln, \$10.00—Hamilton, \$20.00—Jackson, \$50.00—Grant, \$100.00—Benjamin Franklin, \$500.00—McKinley, \$1,000—Cleveland, \$5,000—Madison, \$10,000—Salmon P. Chase. Great Britain has the queen on the front of all bills while the reverse carries: £1—Newton, £5—Duke of Wellington, £10—Florence Nightingale or the British Lion, £20—Shakespeare. Italy has: L 1,000—Verdi, L 5,000—Columbus, L 10,000—Michelangelo, L 50,000—Leonardo da Vinci, L 100,000—Manzoni. French bills, unlike the others, change fairly often, primarily to discourage counterfeiters (Napoleon was on the 100-franc note a few years ago, Voltaire and Hugo are moving out of circulation). When the Banque de France decides that a new note is needed, it commissions several artists to submit proposals and drawings, among which the director of the Banque de France chooses a figure for the new issue. The only guideline seems to be to avoid individuals who would arouse strong disapproval in official circles, in other words, who would be inappropriate cultural carriers (Banque de France, personal communication).

the names of men (and a few women) of letters. In comparison only 2.1% of West Berlin streets are named after writers, 1.9% in East Berlin, 1.2% in Boston, 1% in San Francisco, 0.9% in Chicago and in London, and 0.6% in New York. (Elimination of lettered and numbered streets and inclusion of possible but doubtful names altered the results only slightly.) By the late 19th century, when these cities added and named streets, the French could boast of a long and distinguished literary heritage as the Americans could not. Thus foreign writers name 79% of Chicago's literary streets, 71% of San Francisco's, and 32% of New York's, but account for only 5% of Parisian literary streets.[12]

It is true, of course, that the 332 favored writers in Paris include a fair number of luminaries whose lights are at present dimmed if not altogether extinguished. Still, well-known writers are in sufficient number to give a literary cast to many a Parisian promenade. Other Americans of a literary bent must have reacted like Edmund Wilson who thought it "quite wonderful" to find a street in Paris named for one of his favorite authors and like him must have reflected on the very different situation at home where the efforts to rename Washington Place for Henry James, who was born there, came to absolutely naught (Wilson 1971, p. 156).

These many activities, particularly on the part of the national government (the municipal government in Paris is far less autonomous than in American cities), bespeak a certain if symbolic attention paid to literature, and this attention is by definition that of an elite. For it is not the man in the street who names the street, nor the woman paying for metro tickets who picks the cultural heroes for bank notes, nor the viewer who presents television programs. These decisions translate, directly or indirectly, tastes and preferences not of all consumers but of government elites, while most of the literary prizes, the subjects of *L'Express,* the language and literature columns of *Le Monde* and *Le Figaro,* point to the generalized elite associations of literature in France.

These elite literary preferences affect more than elites. Clearly they serve to weaken other, nonelite definitions of literature. Until recently the literary market in France seemed faintly antiquated, at least from an American perspective. Despite technical advances and a greatly expanded market, large-scale commercialization came to France relatively late, and one reason surely lies with the perception of publishers and booksellers alike that theirs was a "noble" task, hence removed from the vulgar commercialism of the

[12] These figures are based on examination of city guide books or maps. The almost exclusive use of surnames in American cities makes identification considerably more difficult than in Paris or Berlin where the given name is frequently specified. Consultation of a dictionary of Paris streets (Hillairet 1963) corrected the original estimates, increasing the number of writer streets from 282 (5.3% of an estimated 5,418 streets) to 332 (6.4% of a definite 5,218 streets). While similar research might uncover more writers in other cities as well, it would be unlikely to alter the relative positions.

marketplace. Inexpensive paperback books with a mass distribution in France followed England by 30 years, the United States by 20, and even today populistic practices are dubbed with appropriately foreign names like "le discount" and "le best-seller." The opening of a large discount bookstore in Paris (FNAC) provoked cries of outrage and dark intimations that such un-French and unliterary practices portend the demise of French literature itself. Even more "noble" than booksellers, writers are adventurers, to use André Malraux's term, engaged in a calling rather than a mere occupation (Gouillou 1975).

The entrenched elite orientation of French literary culture has affected the diffusion of books as well as their production. Public libraries, to take the most obvious example, ought to counter elitism since diffusion to a broad public is after all their *raison d'être.* It is then striking, and quite consonant with the elite associations of French literary culture, that France should be relatively poor in public libraries. With one-fourth the American population France has only one-tenth as many public libraries: 837 in 1971 versus 8,337 in the United States in 1974 (UNESCO 1973, 1975).[13] The United States has 13 libraries with more than 4 million volumes; France has only one, the Bibliothèque Nationale, which has 7 million volumes but which, unlike the U.S. public libraries (6 out of the 13), is open principally to scholars. Most of the other important libraries in Paris (Senate, National Assembly, Ministry of Foreign Affairs, the Sorbonne, and others) serve primarily individuals associated with the institution, although once again researchers may obtain permission to enter. One did not find in France the distinctive combination of *richesse oblige* and missionary zeal (and ostentation) that prompted American elites to found libraries and other cultural institutions, from museums to universities, designed for general rather than strictly private consumption.

IV

The literary preferences of French elites and the consequent interaction between literary and other elites have sustained characteristic patterns of sociability and combativity in French literary culture, and these have in turn

[13] A general encyclopedia, *Quid* (Frémy and Frémy 1976), all of whose sources I have not checked (but which is reliable on those I have been able to verify), indicates that 20% of Americans are registered in a public (municipal) library versus 4.5% of the French and 30% of the English and Danes. In France 0.75 books are loaned annually per inhabitant versus 5.4 in the United States, 10 in Great Britain, and 7 in Denmark. France spends 2.5 francs ($.58) annually per capita on libraries versus F 37 ($8.51) for Denmark, F 18 ($4.14) in the United States. Dumazedier and Hassenforder (1962) document the underdevelopment of the library system in contrast to the United States, and especially England and the Scandinavian countries. The French government has been making a decided effort to augment public library resources of which the most spectacular example is the open stack public library at the Centre G. Pompidou (Beaubourg).

reinforced certain features of French literature. The virtually unavoidable personal interaction of writers both with elites and among themselves, at court, in salons, cénacles, cafés, frequently over a convivial dinner, supper, or tea, have tended to define literary activities as public, social events as well as, at times perhaps more than, an individual enterprise (see Thibaudet 1940). It is not surprising that this sociability and the conversation that goes with it were what Mme de Staël missed most during her exile from Paris, or that, despite the disappearance of the institutions that originally brought elites together, Parisian literary life is still, or can be if the individual so chooses, very social, very *mondaine.*

The strong ties of literature to traditional elites created definite though subtle constraints to a normative taste (see Renan 1860; Arnold [1864] 1962). At the same time, the very strength of the literary norms provoked equally strong reactions, which is why literary change has so often proceeded by attack and counterattack in a typically French pattern of innovation through highly vocal, often intemperate, rupture with the past. The literary prizes that proliferated in the 19th century intensified this competitiveness to the point where a writer like Zola naturally thought in military metaphors: "Force is everything in the battle of letters." Because many literary groups were small, cohesive, and more or less expressly established to defend a particular conception of literature, they acted to constrain individuals to define their aesthetic and ideological positions and certainly contributed to the intellectualization of French literary life. The exceptional number and concentration of these literary groups and institutions presented so many obstacles for the asocial outsider, who risked neglect by that fact alone. The inescapable confrontations of rival groups, each maneuvering in a small space to impose its own definition of the literary situation, exacerbated the conflicts inherent in any literary culture. "I am attacked, therefore I still am," affirmed Zola, more bellicose than most, no doubt, yet representative of the combative intellectuality of a literary culture which savors aggressive manifestos, ringing critical prefaces, dramatic *causes célèbres,* and resounding *coups de théâtre.*

The normative constraints of French literary culture as well as its intellectuality can be related to France's notably long and continuous literary tradition (Odin 1895; Kroeber [1944] 1963, pp. 536–41).[14] At the same time, if French literary history has no really "off" periods, so to speak,

[14] This seemingly francophile assertion requires some explanation. Other European literary traditions might be as long and/or as rich as the French but their achievements tend to be clustered and not spread so evenly over time. The exceptional continuity of the French literary tradition made French writers especially suitable for Odin's study of the social factors influencing genius (1895, p. 283). Kroeber ([1944] 1963) traced patterns of creativity from the Orient to the Occident over several centuries. Following the conventional ratings of cultural "digests" like textbooks and encyclopedias (p. 23), Kroeber concludes (p. 236) that "from before 1100 on French literature manifests a continuity of performance unequaled in Europe."

neither does it boast any individual who towers over all others, a monumental figure on the order of Dante, Cervantes, Shakespeare, or Goethe. French literature revolves around groups or periods, not individuals. The literary historian will speak of the Age of Louis XIV, not the Age of Corneille or Racine or Molière or whomever, and in a sense the tradition as a whole is more significant than any individual in it (Curtius 1932, p. 189). The many competing literary groups encourage intellectual quality and simultaneously discourage lyric effusions, indeed extremes of any kind. Not surprisingly, many observers have contrasted the French talent for prose with the English gift for poetry—the English poet A. E. Housman once even asked André Gide why French literature had no poets at all!

American elites give no such dramatic coherence to American literary culture. There are, of course, important connections between literature and elites, but they are looser and less conspicuous than in France. Despite the undeniable importance of New York, elites, like writers, are dispersed. Moreover, no entrenched tradition of governmental patronage gave the American writer a sense of importance and of belonging to the center of society rather than being relegated to the periphery. To be sure, there are instances of governmental patronage of letters (Charvat 1968) and in 19th-century Boston the writer was "both the favourite son and the spokesman of the city of culture" (Green 1967, p. 79) where, in the words of one contemporary, "to be known as an able writer is to have the choicest society opened to you" (Green 1967, p. 72). But Yankee Boston is usually considered something of an exception to American mores, if not an aberration. More recognizable, more "American," are the patterns of affiliation characteristic of the Chicago literary world where newspapers exercised much more influence than genteel circles (Duncan 1961).

The many French literary groups which serve as buffers between the writer and mass culture are also fewer or more scattered in the United States. Consequently, the American writer is more at the mercy of popular culture (which may have something to do with the virulence of the so-called mass culture controversy of the 1950s for which there was no French equivalent). The gulf between "serious" (elite by one criterion or another) and popular literature also seems less broad in the United States than in France, perhaps simply because intellectual (and other) constraints are fewer and elites dispersed.

American support for literature has come almost entirely from the private sector, from the new commercial elites of self-made men instead of from an aristocracy which gravitated around the crown. Literature found support through foundations, prize associations, libraries, and eventually universities, which are the largest single source of American literary prizes (29.8%) (no French university sponsors a prize) and which have provided employment for writers ever since Longfellow was appointed to Bowdoin, then

Harvard. Because a French university chair requires the *doctorat d'état* (acquired with considerably more difficulty than a Ph.D.), even writers with advanced degrees like the *licence* or *agrégration,* when they taught, did so at the secondary level. Partly by virtue of this exclusion, partly because of a pervasive distrust of modern literature, the French university never welcomed even the brightest and best of contemporary writers or writing. Only within the past decade have *doctorat d'état* candidates been permitted to write theses on living writers. Creative writing courses, programs, even advanced degrees, have no French equivalent. It is the pluralism of American literature which often strikes French observers (among others), the multiple voices with which it has always spoken, the significance of regional and local differences, the openness, even in academic circles, to popular genres like the detective novel, the western, or science fiction. Because its elite connections are so conspicuous and so ingrained, and because the resulting literary consciousness is highly developed, French literary culture has seemed more of a piece and more elitist than American literary culture.

V

Clearly the centripetal tendencies of French society have reinforced those of French literary culture, while the centrifugal forces of American society have tended to deflect elite tendencies and to reinforce local over national standards. New York has never gone unchallenged, and that challenge has probably been amplified since the National Endowments for the Arts and the Humanities were established in 1965. Not only has there been a certain displacement of literary concerns to Washington, but more important still, the endowments have actively promoted state arts and humanities councils (Netzer 1978, pp. 89–93). The concentration of literary activities in Paris, which facilitates and almost requires personal contact among writers, has done much to sustain a sense of national literary community largely missing in the United States (see Bellow 1977).

Not even New York approaches the dominance Paris has for French letters.[15] Surely a good deal of the excitement writers have found there is generated by the sheer concentration of literary people and resources. Tele-

[15] Analysis of 1970 Census data for 25,376 employed authors showed that 17.3% resided in New York City, 9.1% in Los Angeles, 8.1% in Washington, D.C., 2.9% in San Francisco, and 2.3% in Chicago. By state the range went from 19.97% of authors in New York and 17.55% in California to 5.13% in Maryland and 4.94% in Massachusetts (NEA 1977, pp. 47, 58). Since the "employed author" classification includes professional writers outside the traditional fine arts occupations (NEA 1977, p. 12) one should weight New York more heavily. Nevertheless, if New York dominates the literary scene, it does not stand alone or even in the majority. The selective *Directory of American Poets* of 1975 (Poets and Writers 1975)—a panel evaluates all applications for listings—indicates that 24.5% (330) of the 1,536 listed poets resided in New York City, 13% (201) in New York State outside New York City, 6% (92) in Massachusetts, and 2%–3% in a number of states (Ohio, North Carolina, Michigan, Illinois, Pennsylvania, Connecticut).

phone directories listed 455 bookstores in New York, 431 in London, 337 in Chicago, but 1,239 in Paris. There is a similar concentration of publishers. Telephone directories listed 584 book publishers in Manhattan, 432 in London, 154 in Chicago, but 1,124 in the Paris region, or 74% of the 1,514 publishers in France (289, or 19%, are in the fifth, sixth, and seventh *arrondissements* alone.) Of the 371 largest French publishers (those with sales over $20,000), 321 (87%) are in the Paris region, 302 (81%) in the city itself (Troubnikoff 1972, p. 123), whereas the whole of the Middle Atlantic region, according to the *Census of Manufactures* (U.S. Bureau of the Census 1971), has no more than 42% of all American publishers (which account for 55% of all sales).

Traditionally, a common social and educational background reinforced the connections of literary and other elites in France. Like French elites generally, literary elites came from existing elite milieus (Girard 1961, pp. 89–97; Escarpit 1964, pp. 44–47)[16] and received a common education preparing for the *baccalauréat* (Girard 1961, p. 104; Prost 1968, pp. 58, 245, 331–32). Unlike the American high school diploma received at about the same age, the *baccalauréat* was an elite degree: only 1.35% of boys between 11 and 17 attended a lycée or collège in 1850, 2.59% in 1900, 3.58% in 1930 (Isambert-Jamati 1970, p. 376).

The lycée program, which was determined in detail by the Ministry of Education, was particularly important in inculcating familiarity with and respect for literature. The philosophical and literary component was dominant; the plastic or musical arts received little attention (Ardagh 1968, p. 320; Isambert-Jamati 1970, pp. 380–81). French was taught through Latin models, and Latin, along with the general culture it signaled, both united elites and distinguished them from everyone else (Goblot [1925] 1967). The stress on development of a "correct" style, on effective oral and written expression through the imitation of literary models (Genette 1966), could not but convey the prestige of the acknowledged masters of such expression—writers. The diversity of the American educational system precluded any such common educational (and social) experience.

Political structures reinforced the elite orientation of French society and, by extension, French literary culture. Far more than their American counterparts and well beyond the ancien régime, French political institutions favored the dominance of national elites,[17] whereas the considerable auton-

[16] Of 623 French *gens de lettres* from 1300 to 1825, 26% came from the aristocracy, 30% from the magistrature (judiciary and public administration). Aristocrats declined from 50% of the total for the period 1330–1550 to 16% for 1776–1825 (Odin 1895). For government service in particular, two samples of 19th-century French and English writers by Escarpit (1964, p. 44) show that 16% of the French writers were diplomats or high administrative officials versus only 8% of the English.

[17] A brief trial of universal (manhood) suffage early in the First Republic (1792–99) was succeeded by the Napoleonic Consulate (1799–1804) and authoritarian (First) Em-

omy of American local and state governments supported strong regional identities and provoked suspicions of, rather than identification with, national elites. Especially where governmental interests were at stake, elite notions of any sort tended to be defused by a populism recurrent in American life. Jacksonian "democracy" effectively quashed John Quincy Adams's dream of a national academy of arts and sciences, and when Secretary of State Sumner made a similar proposal in the 1860s, it was at best an anomaly.

VI

The past quarter century has considerably modified these traditional patterns of support. Interest in and a certain sympathy for cultural activities have broadened and created pressures to which the changed and changing tenor of French as of American society has permitted and even forced response. The once very great distance between French and American literary cultures has diminished accordingly.

Modernization of French industry and the economic boom of the 1950s and 1960s could not but affect the production and marketing of books. The mergers of many small (and not so small) publishing houses effected a certain rationalization of commercial procedures, and the image of the writer as adventurer is gradually fading before that of the "professional" in the lay sense for whom specimen contracts, regular royalty statements and disbursements, and social security affiliation may well become the rule rather than the exception.

Democratization of French political institutions (the first direct popular election of the head of state occurred in 1962), often strident criticisms of the status quo coming from within the government as well as from without, the *événements* of 1968 with their dramatic contestation of cultural and other authority, and the coalitions on the Left joined to make the government more sensitive to such popular demands. In its cultural policy the Fifth Republic has sought actively to democratize cultural activities and to attenuate their ingrained elite associations. André Malraux, France's first minister of culture, went so far as to speak of the cultural "rights" of all the French.

But it is the extension of education, of higher education in particular,

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pire (1804–14). During the Bourbon Restoration (1814–30) suffrage was based on property taxes which made 1% of the adult male population eligible to vote. The July Monarchy (1840–48), while keeping the principle of tax-based eligibility, lowered the minimum to include 8% of adult males—and this after a revolution. In both regimes eligibility for election required a still higher tax base. After the brief euphoria of the Second Republic (1848–52), the Second Empire (1852–70) returned to autocracy, and even the Third Republic (1870–1940), which had universal manhood suffrage, remained fairly elitist in its early years. French women did not vote until 1945, a quarter of a century after American women.

that is the essential force in the rapprochement of the two literary cultures. In France the lycées and collèges, which in 1951 included only 8.28% of the 11–17 age cohort (male), expanded to 19.65% by 1964, with a mean annual growth rate of 8.6% in 1960–64 alone (Isambert-Jamati 1970, pp. 376–77). Higher education has followed suit with an average annual compound growth rate in enrollment between 1960 and 1970 of 11.2% in France and 8.3% in the United States (OECD 1974, p. 32). The estimated change in average per capita educational attainment of the population aged 25–64 in France was 3.2% from 1948–58, 3.1% in 1958–68, and an estimated 6.1% for 1968–78; in the United States 2.7% for 1950–60, 3.1% for 1960–70, and an estimated 5.6% for 1970–80 (OECD 1974, pp. 60–62).

In sheer numbers alone students constitute a formidable reading public, not only for textbooks but for works of all kinds. From 282,400 full-time students in higher education in 1960 the French student population almost tripled by 1970 to 846,200; the American student population in the same years more than doubled from 2,466,000 to 5,490,000 (OECD 1974, p. 20). And these figures do not count the part-time students, the habitués and hangers-on in university communities from Berkeley to the Left Bank.

The expanded reading publics have affected publishing in both countries. From 1880 to 1960 the number of titles published annually grew in the United States at an average rate of 2.5% and attained an annual growth rate of 7.8% from 1960 to 1969 (Hokkanen 1969, p. 66). French publishing also increased its production at an annual rate of 6.6% between 1960 and 1970 (Troubnikoff 1972, p. 107). Given the consistent correlation of reading with education, this growth in the educated population bodes well for the recruitment of readers in years to come. However, these "new" readers, increasingly from lower socioeconomic groups, will not resemble the traditional lettered minority. The elite educational patterns once so salient in France will be greatly altered as will reading preferences. Even now popular literature seems in a stage of redefinition as semirespectable. Literary prizes already exist for detective novels and science fiction, and comics cannot be far behind.

In the United States an extension of sympathy, even a demand, for cultural activities coupled with strong support within the federal government culminated in the establishment of the National Endowments for the Arts and the Humanities (20% of NEH funds can be considered support of the arts [Netzer 1978, p. 76]). From an initial budget of $2.5 million, in just a decade each endowment increased its appropriation to $74.7 million. (Appropriations for the 1979 fiscal year are $139.6 million.) During the same period, and partly under NEA and NEH sponsorship, state councils for the arts and the humanities also increased in numbers and in funding (Netzer 1978, pp. 89–93). Even the U.S. Conference of Mayors has an Arts Task Force. However, because American support is recent and also because Ameri-

can legislators are highly sensitive to charges of elitism, more care is taken (and is needed) to justify appropriations and even grants than is the case in France where centuries of funding legitimate government subsidies of the arts.[18]

This notable rapprochement notwithstanding, French and American literary cultures are distinct. Many basic social structures of French and American society remain familiar if not intact. Paris is still the center of French society and of French literary culture, still the locus of French elites. Despite efforts to decentralize funding, the French government continues to dominate patronage of literature as of the arts generally, while the American government walks gingerly on the same path. Education in France remains markedly more elitist than in the United States, primarily because recruitment is more selective: 19.5% of 19-year-olds, 16.2% of 20-year-olds in France were enrolled in institutions of higher education in 1970; the analogous figures for American students are 40.9% and 35.4% (OECD 1974, p. 30).

VII

Not all of society or all of literature equally influences literary culture. Originally the province of an elite, literature, like the arts more generally, has remained dependent on elite support even, or especially, in the era of the mass market. And it is the diverse ties between literary activities and elites which inform, circumscribe, and define literary culture. If literary matters in France often turn into public affairs, this extraliterary resonance is largely a function of the multiple literary associations of national and particularly governmental elites. These ties and associations sustain a generalized elite concern for literary activities, the marked intensity of which contrasts sharply with the more diffuse and, as it were, more private literary ties of American elites.

It is undoubtedly true that the elite ties of literature in France are not unique. Some of the features ascribed to French literary culture are perhaps as European as they are French. Such similarity is scarcely surprising. After all, France shares much with England, Germany, perhaps Western European countries generally: a common legacy of aristocratic patronage of literature and the arts, the congruence of literary and political elites characteristic of

[18] Netzer (1978, pp. 73, 75) estimates that 43% of all NEA grants from 1971–74 were designed to increase the availability of the arts. Without the matching grants from private contributors, the proportion for 1975 and 1976 would be 50%. Similarly, after 1974 NEH shifted its emphasis to its Division of Public Programs, which stressed diffusion to a broad audience. As their budgets increase the endowments become increasingly embroiled in congressional politics (NEA has been honored by one of Senator Proxmire's Golden Fleece awards), hence increasingly subject to local and regional requirements as well as to demands for the "popularization" of the arts.

aristocratic societies, elite educational patterns that stressed literature and the classics (Armstrong 1974, chap. 9), important involvement of current governments in subsidizing literature and the arts. It is only to be expected that literary cultures in these countries should resemble the French pattern more than the American.

But each of these countries is also heir to a very specific set of social, political, economic, literary, and linguistic traditions which necessarily create distinct, and to some extent unique, relations between literary activities and elites. The decentralization of German society and culture, for example, recalls the United States more than France. Likewise, the important hiatus in England between the end of aristocratic patronage of letters in the 18th century and the commencement of government support in the 20th century created circumstances closer to the American than to the French context. The literary cultures sustained by these and other indigenous traditions cannot but be distinct, nearer the French than the American model no doubt, but not identical with it.

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A Brief Guide to the Causal Analysis of Data from Surveys[1]

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Appropriate methods for the causal analysis of certain kinds of systems of quantitative variables have been available for some time now, and they provide useful tools for the research worker in sociology, and in other fields as well, who may be concerned with the study of quantitative variables. Appropriate methods for the causal analysis of corresponding kinds of systems of qualitative variables have been introduced more recently, and they provide useful tools for the research worker who may be concerned with the study of qualitative variables. In order to facilitate the application of these more recently developed methods to the causal analysis of qualitative variables, further exposition of these methods is presented in this note. One of the contributions of this additional exposition is the correction of incorrect statements on this topic by H. T. Reynolds.

The utility of the methods discussed herein will be illustrated by applying them to data obtained in two well-known surveys. We shall reexamine some data on student attitudes analyzed earlier by Coleman (1964) and Goodman (1973*a*) and also some data on voting intention analyzed earlier by Lazarsfeld (1948), Lipset et al. (1954), Campbell (1963), Lazarsfeld, Berelson, and Gaudet (1968), Boudon (1968), Lazarsfeld (1970), and Goodman (1973*a*). These data are reproduced here as tables 1 and 2, respectively. Table 1 cross-classifies the responses of 3,398 schoolboys, each interviewed at two successive points in time, with respect to questions about their self-perceived membership in the "leading crowd" (in it or out of it) and their attitude concerning it (whether they think that membership in it does not require going against one's principles sometimes or whether they think that it does); and table 2 cross-classifies the responses of 266 people, each interviewed at two successive points in time, with respect to their vote intention (Republican vs. Democrat) and their opinion of a particular candidate (for or against the Republican candidate). In the present note, special attention will be given to the causal analysis of these data, using rather simple systems of equations which can be represented, in part, by

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TABLE 1

OBSERVED CROSS-CLASSIFICATION OF 3,398 SCHOOLBOYS, IN INTERVIEWS AT TWO SUCCESSIVE POINTS IN TIME, WITH RESPECT TO TWO DICHOTOMOUS VARIABLES*

		Second Interview			
Membership		+	+	−	−
Attitude		+	−	+	−
First Interview					
Membership	**Attitude**				
+	+	458	140	110	49
+	−	171	182	56	87
−	+	184	75	531	281
−	−	85	97	338	554

SOURCE.—Coleman 1964.

NOTE.—With respect to (self-perceived) membership in the "leading crowd," being in it is denoted + and being out of it is denoted −; with respect to attitude concerning the "leading crowd," a favorable attitude is denoted + and an unfavorable attitude is denoted −. Membership and attitude will be denoted by the letters *A* and *B*, respectively, for the first interview; and by the letters *C* and *D*, respectively, for the second interview.

* Variables = (1) self-perceived membership in the "leading crowd"; (2) favorableness of attitude concerning "leading crowd."

TABLE 2

OBSERVED CROSS-CLASSIFICATION OF 266 PEOPLE, IN INTERVIEWS AT TWO SUCCESSIVE POINTS IN TIME, WITH RESPECT TO TWO DICHOTOMOUS VARIABLES*

		Second Interview			
Vote Intention		+	+	−	−
Candidate Opinion		+	−	+	−
First Interview					
Vote Intention	**Candidate Opinion**				
+	+	129	3	1	2
+	−	11	23	0	1
−	+	1	0	12	11
−	−	1	1	2	68

SOURCES.—Lazarsfeld 1948; Lipset et al. 1954; and Lazarsfeld, Berelson, and Gaudet 1968, among others.

NOTE.—With respect to vote intention, + and − denote Republican and Democrat, respectively; with respect to opinion of a candidate, a favorable opinion is denoted + and an unfavorable opinion is denoted −. Vote intention and candidate opinion will be denoted by the letters *A* and *B*, respectively, for the first interview; and by the letters *C* and *D*, respectively, for the second interview.

* Variables = (1) vote intention; (2) candidate opinion.

path diagrams of the kind displayed in figures 1 and 2, pertaining to tables 1 and 2, respectively.[2]

In figure 1, variables *A* and *B* pertain to self-perceived membership in the "leading crowd" and attitude concerning the "leading crowd," respectively, in the first interview; *C* and *D* pertain to the corresponding variables in the second interview. In figure 2, variables *A* and *B* pertain to vote intention and candidate opinion, respectively, in the first interview; *C* and *D* denote the corresponding variables in the second interview. Figures 1 and 2 pertain to the situation in which variables *A* and *B* are viewed as being prior to *C* (in the sense that the observed level of *A* and *B* might affect the odds pertaining to *C*); and variables *A*, *B*, and *C* are viewed as being prior to *D* (in the sense that the observed level of *A*, *B*, and *C* might affect the odds pertaining to *D*). In these figures, the prior variables *A* and *B* can be viewed as being mutually related to each other, or as being variables that affect each other.[3] In figure 1, there is a main effect of variable *A* on *C*

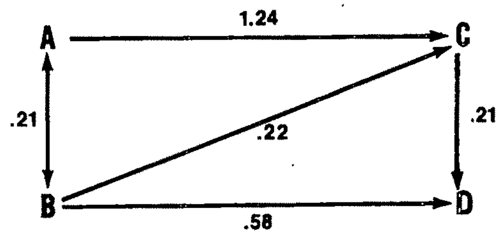

FIG. 1.—Path diagram for the system of eqq. (1a), (1b), (3), and (6), applied to table 1

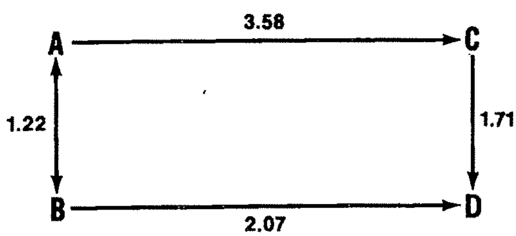

FIG. 2.—Path diagram for the system of eqq. (1a), (1b), (4), and (6), applied to table 2

[2] Figs. 1 and 2 display path diagrams corresponding to certain recursive systems for the qualitative variables *A*, *B*, *C*, and *D*. The remarks on causal analysis in Reynolds (1977) were limited to the analysis of recursive systems, and one of the contributions of the present note will be the correction of incorrect statements on this topic in that article. Special attention will be devoted here to the analysis of recursive systems, but the analysis of nonrecursive systems will also be commented on later herein.

[3] The double-headed arrow between variables *A* and *B* in figs. 1 and 2 can represent either (*a*) the mutual relationship between these two prior variables or (*b*) the effects both of variable *A* on *B* and of variable *B* on *A*. (When the prior variables *A* and *B*

and a main effect of B on C, and there is no interaction between these effects on C; in addition, there is no main effect of A on D, there is a main effect of B on D and a main effect of C on D, and there is no interaction between these effects on D. Figure 2 includes the same kinds of effects as in figure 1 except that there is no effect of variable B on C in figure 2.

To determine whether a specific causal model is congruent with the observed data, the observed frequencies in the table are compared with the expected frequencies estimated under the model, and an assessment is made using the usual goodness-of-fit χ^2 statistic or the corresponding χ^2 statistic based upon the likelihood ratio.[4] When the causal model represented by figure 1 is applied to table 1, the likelihood-ratio χ^2 turns out to be 4.06 with 6 degrees of freedom; and when the causal model represented by figure 2 is applied to table 2, the corresponding χ^2 turns out to be 1.61 with 7 degrees of freedom. Thus, these causal models fit the observed data very well indeed. Since each of these causal models consists of three component models (namely, model H_1 pertaining to the relationship between variables A and B; model H_2 pertaining to variable C with A and B viewed as being prior to it; and model H_3 pertaining to variable D with variables A, B, and C viewed as being prior to it), each of the χ^2 values reported above can be partitioned into its three component χ^2s. This partitioning is summarized in table 3.

Model H_1 in table 3 pertains to the mutual relationship between variables A and B in the two-way table $\{AB\}$. Here the saturated model for table $\{AB\}$ could be considered, or we could consider instead various unsaturated models for table $\{AB\}$. The saturated model can be described by the following formulae for the odds $\Omega_i^{A\bar{B}}$ pertaining to variable B and the odds $\Omega_j^{B\bar{A}}$

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are viewed as being mutually related, the association between these variables is considered to be due to unobserved sources, which are not included explicitly in the model, in addition to the possible influence of variable A on B and/or variable B on A; whereas when the prior variables are viewed as variables that affect each other, the relationship between the variables is considered to be due to the effects of A on B and of B on A.) If variable A had been viewed as being prior to variable B (in the sense that the observed level of A might affect the odds pertaining to B), then a single-headed arrow would have been pointed from A to B; and if variable B had been viewed as being prior to variable A (in the sense that the observed level of B might affect the odds pertaining to A), then a single-headed arrow would have been pointed from B to A. The results presented herein can be directly applied both to the case in which a single-headed arrow is pointed from A to B and to the case in which a single-headed arrow is pointed from B to A, as well as to the case in which there is a double-headed arrow between A and B.

[4] For the sake of brevity and simplicity, this note will not include numerical results for both the goodness-of-fit χ^2 and the likelihood-ratio χ^2. Numerical results for only the latter χ^2 will be presented here. The interested reader can find numerical results for both χ^2 statistics in Goodman (1973*a*, 1973*b*), and he can also find there the appropriate formulae for calculating these quantities as well as the appropriate formulae for calculating the expected frequencies estimated under the causal model.

pertaining to variable A:

$$\Omega_i^{A\bar{B}} = \gamma^{\bar{B}}\gamma_i^{A\bar{B}} , \tag{1a}$$

$$\Omega_j^{B\bar{A}} = \gamma^{\bar{A}}\gamma_j^{B\bar{A}} ; \tag{1b}$$

and the unsaturated models for these odds could be described in similar terms (see, e.g., Goodman 1973*a*, 1973*b*). In both figures 1 and 2, model H_1 for table $\{AB\}$ was taken to be the saturated model.

Model H_2 in table 3 pertains to variable C with A and B viewed as being prior to it. Here the saturated model for the three-way table $\{ABC\}$ could be considered, or we could consider instead certain kinds of unsaturated models for table $\{ABC\}$. The saturated model can be described by the following formula for the odds $\Omega_{ij}^{AB\bar{C}}$ pertaining to variable C:

$$\Omega_{ij}^{AB\bar{C}} = \gamma^{\bar{C}}\gamma_i^{A\bar{C}}\gamma_j^{B\bar{C}}\gamma_{ij}^{AB\bar{C}} ; \tag{2}$$

and the unsaturated models that we might consider for the odds $\Omega_{ij}^{AB\bar{C}}$ could be described in similar terms. In figure 1, model H_2 for table $\{ABC\}$ was taken to be

$$\Omega_{ij}^{AB\bar{C}} = \gamma^{\bar{C}}\gamma_i^{A\bar{C}}\gamma_j^{B\bar{C}} , \tag{3}$$

and in figure 2, model H_2 for table $\{ABC\}$ was taken to be

$$\Omega_{ij}^{AB\bar{C}} = \gamma^{\bar{C}}\gamma_i^{A\bar{C}} . \tag{4}$$

Model H_3 in table 3 pertains to variable D with variables A, B, and C viewed as being prior to it. Here the saturated model for the four-way

TABLE 3

PARTITIONING OF TOTAL VARIATION UNEXPLAINED BY PATH DIAGRAM IN FIGURES 1 AND 2 APPLIED TO TABLES 1 AND 2, RESPECTIVELY

Source of Variation	df	Likelihood Ratio χ^2
	Figure 1 Applied to Table 1	
1. Variation unexplained by H_1 in $\{AB\}$.....	0	0.00
2. Variation unexplained by H_2 in $\{ABC\}$....	1	0.02
3. Variation unexplained by H_3 in $\{ABCD\}$...	5	4.04
Total variation unexplained by fig. 1.......	6	4.06
	Figure 2 Applied to Table 2	
1. Variation unexplained by H_1 in $\{AB\}$.....	0	0.00
2. Variation unexplained by H_2 in $\{ABC\}$....	2	0.15
3. Variation unexplained by H_3 in $\{ABCD\}$...	5	1.45
Total variation unexplained by fig. 2.......	7	1.61*

* The sum is 1.61, rather than 1.60, when more significant digits are used than are reported here.

table $\{ABCD\}$ could be considered, or we could consider instead certain kinds of unsaturated models for table $\{ABCD\}$. The saturated model can be described by the following formula for the odds $\Omega_{ijk}{}^{ABC\bar{D}}$ pertaining to variable D:

$$\Omega_{ijk}{}^{ABC\bar{D}} = \gamma^{\bar{D}}\gamma_i{}^{A\bar{D}}\gamma_j{}^{B\bar{D}}\gamma_k{}^{C\bar{D}}\gamma_{ij}{}^{AB\bar{D}}\gamma_{ik}{}^{AC\bar{D}}\gamma_{jk}{}^{BC\bar{D}}\gamma_{ijk}{}^{ABC\bar{D}} \; ; \tag{5}$$

and the unsaturated models that we might consider for the odds $\Omega_{ijk}{}^{ABC\bar{D}}$ could be described in similar terms. In both figures 1 and 2, model H_3 for table $\{ABCD\}$ was taken to be

$$\Omega_{ijk}{}^{ABC\bar{D}} = \gamma^{\bar{D}}\gamma_j{}^{B\bar{D}}\gamma_k{}^{C\bar{D}} . \tag{6}$$

The overall causal models presented above, which are described, in part, by the corresponding path diagrams, make explicit what is the assumed order of priority of the variables in the model; and this assumed order of priority will determine which are the relevant multiplicative models to be considered. Thus, for the order of priority assumed above, the relevant multiplicative models to be considered are the saturated models (1a), (1b), (2), (5), and corresponding unsaturated models. These models pertain to the odds $\Omega_i{}^{A\bar{B}}$, $\Omega_j{}^{B\bar{A}}$, $\Omega_{ij}{}^{AB\bar{C}}$, and $\Omega_{ijk}{}^{ABC\bar{D}}$, respectively; and each of the corresponding multiplicative models for the odds can be replaced by the equivalent additive (logit) model for the log-odds (see, e.g., Goodman 1972*a*, 1973*a*).

The numerical magnitude associated with each of the arrows in figures 1 and 2 is the magnitude of the corresponding effect as estimated with the appropriate logit model obtained when the multiplicative models presented above for the odds are replaced by the equivalent additive models for the log-odds. These numerical magnitudes are of substantive interest in understanding the phenomena analyzed with the data in tables 1 and 2. These quantities can be interpreted in a manner similar to the interpretation of the effects in the usual additive model used in the analysis of variance and to the interpretation of the slopes in the usual multiple-regression model (see, e.g., Goodman 1975). Some further comments on these magnitudes will be made later herein.

CAUSAL ANALYSIS AND THE ANALYSIS OF MARGINAL TABLES

In the preceding comments on the causal analysis pertaining to the path diagrams in figures 1 and 2 applied to tables 1 and 2, respectively, we took note of the fact that the assumed order of priority of the variables, which is made explicit in the overall causal model, will determine which multiplicative models (or equivalent additive logit models) are relevant. After this determination of the relevant models has been made, these models would then be applied to the appropriate sets of data. Thus, for example, the

saturated models (1a) and (1b), and corresponding unsaturated models, would be applied to the two-way table $\{AB\}$; the saturated model (2), and corresponding unsaturated models, would be applied to the three-way table $\{ABC\}$; the saturated model (5), and corresponding unsaturated models, would be applied to table $\{ABCD\}$.

The assumed order of priority of the variables determined which models were relevant, and the models in turn determined which tables were relevant.[5] Of course, if the overall causal model under consideration is changed in ways that also change the assumed order of priority of the variables, then there will also be a change in the models that are relevant and in the tables that are relevant (i.e., in the tables to which these models are to be applied).

In the preceding discussion, with the particular order of priority assumed there, we were led to an analysis in turn of the tables $\{AB\}$, $\{ABC\}$, and $\{ABCD\}$, using the relevant multiplicative models for the corresponding odds or the equivalent additive logit models for the corresponding log-odds. There was no need in that discussion to consider other more general log-linear models applied to these tables.[6] Indeed, the more general log-linear models (or the equivalent more general multiplicative models), applied to these tables, would not be particularly relevant in the analysis of recursive systems of the kind we have been considering here.[7] However, the overall causal model pertaining to the analysis of a recursive system, which can be described either in terms of a set of additive logit models for the log-odds or in terms of a corresponding set of equivalent multiplicative models for the odds (e.g., the set of models [1a], [1b], [3], and [6] used to describe the causal model in fig. 1), sometimes turns out to be equivalent to a single log-linear model (or a corresponding single [more general] multiplicative model) applied to the full table $\{ABCD\}$. This was the case for the causal model in figure 1, but it was not the case for the causal model in figure 2. (For further details on these matters, see, e.g., Goodman [1973*a*, 1973*b*].)

Note that the appropriate methods of causal analysis presented above led to an analysis in turn of tables in which certain specified variables were collapsed. In the analysis of the saturated models (1a) and (1b), and corresponding unsaturated models, since table $\{AB\}$ was used, variables C and D were collapsed; and in the analysis of the saturated model (2), and cor-

[5] We can view the assumed order of priority of the variables as determining which models are relevant and also which tables are relevant.

[6] The additive logit model for the log-odds can be viewed as a special case of more general log-linear models (see, e.g., Bishop 1969; Goodman 1970, 1971). Similarly, the multiplicative model for the odds can be viewed as a special case of a more general kind of multiplicative model (see, e.g., Goodman 1972*a*, 1972*b*, 1973*a*).

[7] The more general log-linear models (and the equivalent more general multiplicative models) are relevant in the analysis of nonrecursive systems. Some further comments on these more general models and on the analysis of nonrecursive systems appear later herein.

responding unsaturated models, since table $\{ABC\}$ was used, variable D was collapsed. In the analysis of the saturated models (1a) and (1b), and corresponding unsaturated models, the existence or nonexistence of the possible two-factor effect (AB) between variables A and B in the two-way table $\{AB\}$ is relevant; but the existence or nonexistence of a corresponding possible two-factor effect in the four-way table $\{ABCD\}$ is not relevant when variables A and B are viewed as being prior to variables C and D. Similarly, in the analysis of the saturated model (2), and corresponding unsaturated models, the existence or nonexistence of the possible three-factor effect (ABC) between variables A, B, and C in the three-way table $\{ABC\}$ is relevant; but the existence or nonexistence of a corresponding possible three-factor effect in the four-way table $\{ABCD\}$ is not relevant when variables A, B, and C are viewed as being prior to variable D.

The remarks on this general topic in Reynolds (1977) are incorrect in various respects. Let us now consider some of the errors.[8]

SOME ERRORS

1. Irrelevant Criticism

Reynolds (1977) seems to be criticizing the methods of analysis presented in Goodman (1973*a*) on the grounds that, when these methods are used in the analysis of a specified overall causal model (with a specified order of priority for the variables in the causal model), and the relevant component models for the odds (or the log-odds) are applied to the relevant tables (e.g., tables $\{AB\}$, $\{ABC\}$, and $\{ABCD\}$, with the particular order of priority considered herein), then some of the effects that are included in the relevant component models may correspond to effects that could be omitted from a log-linear model applied to the full table. In particular, Reynolds seems to find fault with these methods when they are used in the analysis of the recursive system represented by figure 1 above (fig. 10 in Goodman 1973*a*) applied to the data in table 1, because these methods lead to the inclusion of the two-factor effect (AB) between variables A and B in the relevant component model pertaining to the two-way table $\{AB\}$, while a corresponding two-factor effect in a log-linear model applied to the four-way table may be less pronounced (or absent). From the results presented near the end of the preceding section, we see that Reynolds's supposed criticism is incorrect, since the existence or nonexistence of the two-factor effect (AB) in the two-way table $\{AB\}$ is relevant in the causal analysis of the recursive system represented by figure 1, whereas the existence or nonexistence of a corresponding effect in the four-way table would not be relevant.

For some other causal models which are very different from the ones

[8] For additional comments on some of the errors, see Gillespie (1978).

considered here,[9] the existence or nonexistence of a two-factor effect (AB) in the four-way table would have some relevance; and these particular models have been discussed in Goodman (1973*a*, 1974).[10]

2. Incorrect and/or Irrelevant Simulation

Reynolds also seems to be criticizing the methods of analysis presented in Goodman (1973*a*) on the grounds that these methods would fail to detect the structure in a recursive system of *quantitative* variables when these variables have been dichotomized. But the structure of the system of dichotomized variables will usually be very different from the structure of the corresponding quantitative variables.[11] The methods of analysis presented in Goodman (1973*a*) and in this article were developed for the analysis of the structure of a system of dichotomous or polytomous variables; they were *not* developed for the analysis of the structure of a system of quantitative variables. (This is true also of the methods of analysis presented in Goodman's articles on related topics; see, e.g., his papers reprinted in Goodman [1978].) In Reynolds's attempt to see whether the methods described in Goodman (1973*a*) could detect the known underlying structure in a simulated recursive system, he incorrectly simulated a system of *quantitative* variables (which he then dichotomized) when he should instead have simulated a system of *qualitative* variables. Reynolds's simulation, presented in his section discussing Goodman's methods for the analysis of recursive systems of qualitative variables, is thus incorrect and/or irrelevant.

In addition to the simulation mentioned above, Reynolds also presented a simulation pertaining to the analysis of conditional independence. Here too he simulated a system of *quantitative* variables (which he then dichotomized and polytomized) instead of a system of *qualitative* variables. But none of Goodman's articles have suggested that the appropriate methods developed for the analysis of *qualitative* variables (e.g., methods developed for the analysis of conditional independence models, logit models, log-linear models, etc.) should be applied to arbitrarily dichotomized (or polytomized) *quantitative* variables. Indeed, the appropriate methods for

[9] In the causal models considered here variables A and B were assumed to be prior to variables C and D, whereas in the other causal models variables C and D would have to be assumed to be prior to variables A and B. Since variables A and B pertain to responses in a first interview, and variables C and D pertain to responses in a second interview, the latter causal models would have to involve apparent time reversal. For a discussion of related matters, see Goodman (1973*a*, 1974).

[10] See, e.g., figs. 3, 4, 8, and 9 in Goodman (1973*a*); fig. 6 in Goodman (1974); and the discussion of models H''_3 and H_{13} in the latter article.

[11] For further comments on the difference between the structure of the system of dichotomized variables and the structure of the corresponding quantitative variables, see the Appendix below.

the analysis of *qualitative* variables should not be applied in this inappropriate way.[12]

3. Incorrect Invocation of Theorem on Collapsing Variables

In Reynolds's section on the analysis of recursive systems, he attempts to explain why, in the analysis of the three-way marginal table $\{ABC\}$ obtained with his simulated data, a two-factor effect (BC) was needed, and he incorrectly invokes Bishop, Fienberg, and Holland's (1975) theorem here. Reynolds states that "certainly we know from Bishop et al.'s theorem that such a term might be expected to appear. Analysis of the $\{ABC\}$ marginal table . . . almost guarantees that we will find such a link" (p. 139). This statement is incorrect, since (*a*) the term appeared in the analysis of the simulated data because the simulation was done incorrectly (see comment on this at the beginning of the preceding section), (*b*) if the simulation had been done correctly the term would not have appeared, (*c*) Bishop et al.'s theorem does *not* imply that such a term might be expected to appear, and (*d*) the analysis of the $\{ABC\}$ marginal table would *not* even "almost guarantee" that there would be such a link.[13]

Similar incorrect invocations also appear in other sections in Reynolds's article. For example, in his section on collapsing variables for the sake of convenience, for the particular data considered there Reynolds states that "when the variables have not been collapsed much simpler models become acceptable. . . . One can see how Bishop et al.'s theorem might account for these results. . . . By simply combining categories one may create artificial interactions which will give a misleading picture of the underlying relationships" (pp. 136–37). In a similar vein, Reynolds also states a little later that "collapsing a variable may require one to fit parameters that would otherwise be unnecessary" (pp. 139–40). In contrast to such statements, it should be noted that collapsing a variable, or combining particular categories of a variable, can lead either to the deletion of parameters that would otherwise be necessary,[14] or to the insertion of parameters that would otherwise be unnecessary, or to no change in the parameters.[15]

[12] For appropriate methods for determining whether particular categories of a polytomous variable can be combined, see, e.g., Duncan (1975).

[13] These brief remarks will be understood more fully by the reader after he has read the comments on the analysis of marginal tables in the final section herein.

[14] With respect to this effect of collapsing a variable, the analysis of table 1 in Goodman (1973*a*) (i.e., table 2 above) illustrated this in a dramatic way, and it will be discussed further in the final section below. (The reader is also referred to Goodman [1972*b*] for results that show in which direction the magnitude of a parameter will change when a dichotomous variable is collapsed [i.e., deleted] or inserted.) With respect to this effect of combining particular categories of a variable, the analysis in Duncan (1975) illustrated this well. The analysis of the effects of collapsing a variable (i.e., of combining *all* the categories of the variable into a single combined category) is quite different from the analysis of

4. Misleading Statements

In several places in his article, Reynolds makes statements that may be misleading and that are incorrect in a different way from the statements noted above. Here is one example.

In the section in which he criticizes collapsing the categories of a variable for the sake of convenience (e.g., dichotomizing a variable for convenience), he states that "in some cases [Goodman] has collapsed variables that originally had more than two categories. . . . The unwary reader may be left with the impression that the number of categories does not matter. He may feel that Goodman's . . . procedures permit him to manipulate data in any way he pleases . . ." (p. 135). This statement may be misleading, since (*a*) in none of my articles have I collapsed a variable that had more than two categories in the main source (or sources) that I used for comparative purposes,[16] and (*b*) these articles do *not* leave the impression that the number of categories does not matter and that the researcher is permitted to manipulate data in any way he pleases.[17]

the effects of combining particular categories of a variable (as long as all the categories are not combined into a single category). This distinction between the analysis of the effects of collapsing a variable and the analysis of the effects of combining particular categories of a variable is an important one. (Reynolds failed to take this distinction into account in his article.)

[15] This comment on the possible effects of collapsing (i.e., deleting) a variable, or combining particular categories of a variable, can also be stated in terms of the possible effects of inserting a variable, or decomposing a particular category of a variable into two or more subcategories. Thus, when a variable is inserted, or when a particular category of a variable is decomposed into two or more subcategories, this can lead either to the insertion of parameters that would otherwise be unnecessary, or to the deletion of parameters that would otherwise be necessary, or to no change in the parameters. The three possible effects listed above will be discussed more fully in the final section herein when the sign-rule in table 4 is considered.

[16] To illustrate the utility of the methods introduced in these articles, and to compare these methods with earlier methods used by others, I reanalyzed data that had been analyzed earlier by others, and I used the variables as they were used by those with whom I was comparing my work. In one of the many examples, I reanalyzed data that had been analyzed earlier by Coleman (1964) and Theil (1970). There were four variables under consideration in that example; three of them were dichotomous and Coleman (1964) dichotomized the fourth. Theil (1970) used the variables as they were used by Coleman in order to compare his method with Coleman's, and I used the variables as they were used by Coleman and Theil in order to compare my methods with both of theirs. (Reference was also made in my article to Zeisel [1968], who presented the fourth variable in trichotomous form, and to the original source of the data in Stouffer et al. [1949].) A somewhat similar kind of remark applies also to the example in which I reanalyzed data that had been analyzed earlier by Davis (1971). In the two illustrative applications referred to above (namely, in the two examples using the data analyzed by Coleman and Theil and by Davis), and in all the other examples in my articles, I used the variables as they were used earlier by those with whom I was comparing my work.

[17] To facilitate the exposition of the illustrative applications presented in this series of articles, almost all of the variables used in these applications were dichotomous; but almost

RECURSIVE SYSTEMS AND NONRECURSIVE SYSTEMS

As noted earlier, the path diagrams in figures 1 and 2 above provide examples of recursive systems; and most of the discussion in the earlier sections in the present note pertained to such systems. Next let us consider briefly nonrecursive systems and their relationship to recursive systems.

In path diagrams of the kind displayed in figures 1 and 2, the level of variables A and B might affect the odds pertaining to variable C, and the level of variables A, B, and C might affect the odds pertaining to D; but the level of D cannot affect the odds pertaining to any of the other variables. Consider next the situation where the level of variables A, B, and C might affect the odds pertaining to D; and the level of variables A, B, and D might affect the odds pertaining to C. In this situation, we have both an equation for the odds $\Omega_{ijk}{}^{ABC\bar{D}}$ pertaining to D (see, e.g., eqq. [5] and [6]) and a similar kind of equation for the odds $\Omega_{ijl}{}^{ABD\bar{C}}$ pertaining to C. Systems of equations of this general kind can be analyzed using related log-linear models, and the methods for doing so have been described in Goodman (1973*a*, 1973*b*). For the sake of brevity, we shall not consider here these systems of equations, but only the related log-linear models.

In order to facilitate comparison with our earlier path diagrams, let us consider two log-linear models: model M_1 which includes the two-factor effects (AB), (AC), (BC), (BD), (CD) in the four-way table $\{ABCD\}$; and model M_2 which includes the two-factor effects (AB), (AC), (BD), (CD) in the four-way table. When these models are applied to table 2,[18] we obtain likelihood-ratio χ^2 values of 1.46 with 6 degrees of freedom and 6.74 with 7 degrees of freedom for models M_1 and M_2, respectively. Comparing the χ^2 values for models M_1 and M_2, we find that M_1 is more congruent with the observed data than is M_2, and the difference between the models is statistically significant.

We next compare the χ^2 value for model M_1 with the corresponding value for the causal model in figure 2: namely, 1.61 with 7 degrees of freedom. The difference between these two χ^2 values is not statistically significant. The causal model in figure 2 is more parsimonious than is model M_1, since there

all of these articles also included appropriate formulae for the more general case where the variables may be polytomous. The reader is referred to Goodman (1971) for an illustrative application in which one of the variables is trichotomous (with the three categories ordered from soft to hard), and also for the presentation of more general methods that are appropriate when each variable may have any given number of categories, with the categories of each variable either ordered in some specified way or left unordered. Of course, the fact that the methods presented in this series of articles can be applied when each variable can have any given number of categories does *not* at all imply that the results obtained with these methods will be unaffected by the number of categories used with each variable.

[18] For the sake of brevity, only table 2 will be considered here. The interested reader can apply these models to table 1 as well (see, e.g., Goodman 1973*a*).

are 7 degrees of freedom pertaining to the causal model in contrast to 6 degrees of freedom pertaining to model M_1.

From the results presented in the two preceding paragraphs, we see that, in the comparison of models M_1 and M_2, because model M_2 did not include the two-factor effect (BC) in the four-way table $\{ABCD\}$ and model M_1 did include this effect, there was a statistically significant difference between M_1 and M_2; whereas, in the comparison of model M_1 and the causal model in figure 2, although the causal model did not include a corresponding two-factor effect between variables B and C in the three-way table $\{ABC\}$, the difference between these two models was not statistically significant. Under the assumed order of priority of the variables in the causal model in figure 2, this causal model will be preferable to model M_1,[19] since the former model is more parsimonious.

THE MAGNITUDE OF THE EFFECTS

We shall now briefly reflect upon the magnitude of the effects among the variables in different models. Thereby, we shall gain some insight into, say, the reason why the two-factor effect between variables B and C in the three-way table $\{ABC\}$ could be deleted, as was done in figure 2, whereas a corresponding two-factor effect in the four-way table $\{ABCD\}$ could not be deleted.[20]

Figure 3 displays the path diagram for the log-linear model M_1 applied to the four-way table (table 2), in the case in which each of the four variables in the table can be affected by the other variables in the table.[21] To

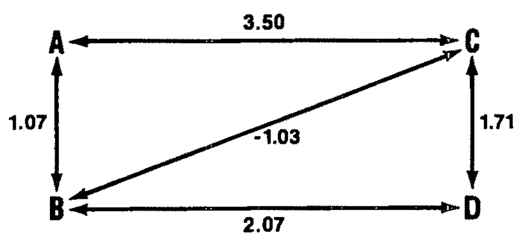

FIG. 3.—Path diagram for the log-linear model M_1 applied to table 2

[19] Under the assumed order of priority of the variables, model M_1 is equivalent to a causal model of the kind displayed in fig. 1 herein. (For further details, see Goodman 1973*a*.)

[20] As noted in the preceding section, with the deletion of the two-factor effect (BC) in the four-way table $\{ABCD\}$, we obtained model M_2 which was not as congruent with the observed data as was model M_1. And the difference between the models was statistically significant.

[21] For the corresponding path diagram obtained when variables A and B are viewed as being prior to variables C and D, see Goodman (1973*a*). For expository purposes, we present in fig. 3 here only the case mentioned in the text.

compare the magnitude of the corresponding effects presented in figures 2 and 3, we can apply some results introduced in Goodman (1972*b*) which show in which direction the magnitude of an effect will change when variables are deleted (i.e., collapsed) or inserted.

Before comparing the magnitudes of the corresponding effects in figures 2 and 3, let us first review the results introduced in Goodman (1972*b*). The following kind of sign-rule was presented there: When there is no three-factor interaction in a three-way $2 \times 2 \times 2$ table $\{ABC\}$, then the two-factor effect (AB) in the two-way marginal table $\{AB\}$ will be larger than, smaller than, or equal to a corresponding effect in the three-way table, if the product of the two-factor effects (AC) and (BC) in the three-way table is positive, negative, or zero, respectively. Table 4 displays the possible consequences derived with this sign-rule. From table 4 we see that, for example, when (AC) and (BC) have the same sign, if the (AB) effect in table $\{ABC\}$ is negative, then this effect will increase when variable C is collapsed so that (AB) will go from being negative to being either less negative or zero or positive. Similarly, we see that, for example, when (AC) and (BC) have different signs, if the (AB) effect in table $\{ABC\}$ is positive, then this effect will decrease when variable C is collapsed so that (AB) will go from being positive to being either less positive or zero or negative. We can now apply here the sign-rule described above and various generalizations of this rule introduced in Goodman (1972*b*).

With the comparison of χ^2 values for models M_1 and M_2 in the preceding section, we found that an effect of -1.03 pertaining to the two-factor effect (BC) in the four-way table $\{ABCD\}$ made a statistically significant contribution, and so we preferred model M_1 (which included this effect) to

TABLE 4

THE SIGN-RULE APPLIED TO THE TWO-FACTOR INTERACTION (AB) IN THE THREE-WAY $2 \times 2 \times 2$ TABLE $\{ABC\}$, WHEN VARIABLE C IS COLLAPSED

Sign of the Interactions in Table $\{ABC\}$		Change in (AB) When Variable C Is Collapsed
(AC)	(BC)	
+	+	Increase
−	−	Increase
+	−	Decrease
−	+	Decrease
0	+ or −	No change
+ or −	0	No change
0	0	No change

model M_2 (which did not include the effect). On the other hand, the -1.03 effect in figure 3 could be replaced by a zero effect when the three-way table $\{ABC\}$ was analyzed as part of the overall causal model in figure 2. Applying a generalization of the sign-rule in Goodman (1972*b*), with the deletion of variable D, the two-factor effect (BC) will increase (from -1.03 to approximately zero) since the product of the (BD) effect and the (CD) effect is positive.[22]

Note that the (BD) and (CD) effects in figure 3 are equal in magnitude to the corresponding effects in figure 2. This is a consequence of the fact that both the log-linear model M_1 and the overall causal model in figure 2 imply the same equation (6) for the odds $\Omega_{ijk}^{ABC\bar{D}}$.[23]

Note also that the (AB) and (AC) effects in figure 3 are somewhat smaller than the corresponding effects in figure 2. This is a consequence of the following facts: With the deletion of variable D, the (AB) and (AC) effects in the three-way table $\{ABC\}$ will be equal in magnitude to the corresponding effects in the four-way table $\{ABCD\}$ since the (AD) effect was zero in figure 3;[24] and as noted earlier in this section, the (BC) effect will be increased from -1.03 to approximately zero. Actually, the (BC) effect in the three-way table $\{ABC\}$ turns out to be close to zero but slightly positive; and when this effect is set equal to zero (as it is in the analysis of table $\{ABC\}$ under the overall causal model in figure 2), then the only two-factor effects in table $\{ABC\}$ are the (AB) and (AC) effects, each of which can then be measured (under the causal model) using the corresponding two-way marginal tables $\{AB\}$ and $\{AC\}$, respectively. The (AB) effect in the two-way table $\{AB\}$ will be larger than a corresponding effect in the three-way table because the product of the (AC) effect and the (BC) effect is positive; and the (AC) effect in the two-way table $\{AC\}$ will be larger than a corresponding effect in the three-way table because the product of the (AB) effect and the (BC) effect is positive.

[22] The negative (BC) effect (namely, -1.03) in the four-way table $\{ABCD\}$ analyzed in fig. 3 can be used to explain why a corresponding effect in the three-way marginal table $\{ABC\}$ is approximately zero; or, conversely, the zero effect between variables B and C in the three-way table $\{ABC\}$, analyzed as part of the overall causal model in fig. 2, can be used to explain why there is a negative (BC) effect in the four-way table $\{ABCD\}$. The latter form of explanation is preferable since the overall causal model in fig. 2 is more parsimonious than is model M_1 in fig. 3.

[23] Model M_1 also implies that there is no three-factor interaction (ABC) in the three-way table $\{ABC\}$, and the overall causal model in fig. 2 implies that variables B and C are conditionally independent of each other, given variable A (see Goodman 1973*a*).

[24] Using a generalization of the sign-rule referred to earlier, the (AB) effect will remain unchanged because the product of the (AD) effect and the (BD) effect will be zero; and the (AC) effect will remain unchanged because the product of the (AD) effect and the (CD) effect will be zero.

APPENDIX

The purpose of this Appendix is to clarify why the structure of a system of quantitative variables will usually be very different from the structure of the corresponding dichotomized variables. To facilitate this exposition, we shall consider the case where the structure of the system of quantitative variables can be described in terms of the concept of conditional independence. Under conditions specified below, if conditional independence holds true for specified quantitative variables in the system, then conditional independence will *not* hold true for the corresponding dichotomized variables.

Let X, Y, and Z denote three quantitative variables, and let X', Y', and Z' denote the corresponding dichotomized (or polytomized) variables, with X, Y, and Z different from X', Y', and Z', respectively. Suppose that X and Y are conditionally independent of each other, given variable Z. For simplicity, we shall consider the case in which $X = a + bZ + \epsilon_1$ and $Y = c + dZ + \epsilon_2$, with $b \neq 0$, $d \neq 0$, and the usual error terms ϵ_1 and ϵ_2 are independent of each other. In this case, with X and Y conditionally independent of each other, given variable Z, we shall find that X and Y will *not* be conditionally independent of each other, given variable Z', *and* X' and Y' will *not* be conditionally independent of each other, given variable Z'; but X' and Y' will be conditionally independent of each other, given variable Z.

To understand why X and Y are *not* conditionally independent of each other, given variable Z', consider first a specified value of Z' and each of the possible values of Z that can yield this specified value of Z'. For the specified value of Z', the conditional joint distribution of X and Y (given this specified value of Z') can be obtained by considering the conditional joint distribution of X and Y obtained for each possible value of Z that can yield the specified value of Z', and then weighting these joint distributions in accordance with the distribution of the corresponding possible values of Z. Thus, in the special case in which, say, two possible values of Z can yield the specified value of Z', then the conditional joint distribution of X and Y (given this specified value of Z') is a mixture of two conditional joint distributions that have different expected values for X (since $b \neq 0$) and different expected values for Y (since $d \neq 0$); and in the mixture thus obtained variables X and Y will be positively correlated when $bd > 0$ and negatively correlated when $bd < 0$.

As noted above, variables X and Y are *not* conditionally independent of each other, given variable Z', when $b \neq 0$ and $d \neq 0$. However, in the special case in which $b = 0$ and/or $d = 0$, then X and Y are conditionally independent of each other, given variable Z'.[25]

[25] To understand why this is so, return to the discussion presented in the preceding paragraph, and note now that the two conditional joint distributions in the mixture described

The comments in the two preceding paragraphs pertain to the analysis of the conditional joint distribution of X and Y, given a specified value of Z'. These comments can also be extended directly to the analysis of the conditional joint distribution of X' and Y', given a specified value of Z'.

Consider next the analysis of the conditional joint distribution of X' and Y', given variable Z. It is easy to see that if two quantitative variables are independent of each other, the corresponding dichotomized (or polytomized) variables will also be independent of each other. (Consider the joint distribution of the two quantitative variables and the corresponding joint distribution obtained from it when the two variables are dichotomized or polytomized.) Thus, when X and Y are conditionally independent of each other, given variable Z, then X' and Y' will also be conditionally independent of each other, given variable Z.

The results presented in this Appendix could have been stated in more general terms, but we shall not go into these matters here.

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there will have the same expected values for X when $b = 0$ and the same expected values for Y when $d = 0$; and in the mixture thus obtained variables X and Y will be independent of each other (when $b = 0$ and/or $d = 0$).

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Structural Position in the World System and Economic Growth, 1955–1970: A Multiple-Network Analysis of Transnational Interactions[1]

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This paper addresses world-system/dependency theories of differential economic growth among nations. We grant that such perspectives have considerable analytic potential but have serious reservations concerning their current empirical status. Our criticisms focus particularly on the absence of evidence on the theoretically specified structural positions (core, semiperiphery, periphery) in the world system and the dynamic relations among them. After indicating why extant quantitative studies that claim to represent "position" are inadequate, we propose that blockmodel analyses of social structure through multiple networks address world-system formulations far more appropriately. We present a blockmodel of the world system circa 1965 that is based on four types of international networks: trade flows, military interventions, diplomatic relations, and conjoint treaty memberships. While we invite replications with additional network data, this blockmodel provides strong evidence for a core-semiperiphery-periphery structure. We then report regression analyses of the effects of these structural positions on nations' economic growth (change in GNP per capita) from 1955 to 1970. Net of other plausible determinants, these effects are large in magnitude and entirely consistent with world-system/dependency theories. Further analyses reinforce the interpretation of these findings as the structural, accumulative advantage of location in the core over that in the periphery. Substantively, our results suggest that exogenetic theories of economic growth are even more powerful than previous analyses have indicated. Moreover, they demonstrate the natural wedding of a conceptual framework (the world system) with an empirically grounded theory of social structure (blockmodel analysis), which has applicability much beyond issues of economic growth.

INTRODUCTION

Differential economic growth among countries constitutes both a central dimension of international stratification and an important determinant of other characteristics (transnational power, individuals' life chances) that

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are delimited by geopolitical boundaries (e.g., Horowitz 1972). The arena of economic growth has now become a widely used testing ground for alternative theoretical approaches that also frequently address broader issues concerning processes of modernization, societal development, etc. (Portes 1976). Recent perspectives emphasizing the "world economy" as the appropriate level of inquiry and the dynamic, exploitative linkages among its constituent political units as a substantive corollary appear to have considerable analytic power (see Wallerstein 1974; Chirot 1977; Portes 1976; Chase-Dunn 1975; Rubinson 1976). These world-system/dependency theories therefore serve as a sharp counterpoint to the ontogenetic assumptions of more conventional developmental treatments (Rostow 1960; Inkeles and Smith 1974; see Portes [1976] and Chase-Dunn [1975] for cogent reviews of both literatures).[2]

While several analyses attempt to adjudicate hypotheses derived from the alternative theoretical models of economic growth (e.g., Chase-Dunn 1975; Delacroix and Ragin 1978), that is not our purpose here. In fact, we treat developmental theories largely as straw men. That is not because they lack any merits. Available arguments and evidence (Portes 1976; Delacroix and Ragin 1978) suggest that national development and economic growth in particular reflect a complex interaction of inter- *and* intranational factors (especially state strategies and educational institutions). Nevertheless, we are in accord with Portes's (1976, p. 80) assessment that "newer perspectives [world-system/dependency approaches] . . . represent a potentially more useful guide for future investigation." An exogenetic vantage point can easily incorporate domestic factors (world-system approaches consider this to be mandatory), while the converse would be conceptually more difficult.

Consequently, we take world-system/dependency analysis as our point of substantive departure in this paper. But the main premise underlying our treatment is that, despite its analytic potential, the empirical status of this

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[2] We are well aware that longstanding theories of imperialism (e.g., Lenin's) underlie world-system/dependency analyses (see Portes 1976; Chase-Dunn 1975; Chirot 1977). However, recent variants of such arguments (Wallerstein 1974; Chirot 1977) are more directly central to our current aims. We also recognize that world-system and dependency theories are not necessarily identical. The latter arguably refer more strictly to certain structural aspects of Latin American nations specified by a group of radical economists from those areas (see McDaniel [1976–77] for a review). These distinctions are again unimportant for present purposes, and other treatments (e.g., Delacroix 1977) consider world-system/dependency analysis as a single line of argument.

formulation remains open to serious challenge. We shall develop in detail criticisms concerning the absence of clear operational criteria for identifying the location of nations in the world system, and the corresponding lack of evidence that exploitative structural relations in that system operate as theoretically specified. We also discuss related problems which characterize recent quantitative studies of the effects of dependency on economic growth in the post–World War II period. We argue that, despite explicit claims to the contrary, these studies do not adequately measure the concepts of "position" and "control" in the world system and do not fully test the relevant hypotheses. We propose instead that the blockmodel analyses developed by White and his associates (e.g., White, Boorman, and Breiger 1976) more closely capture these structural aspects of the world economy. The bulk of the paper reports two separate but related investigations that bear on world-system theory. The first is a multiple-network (blockmodel) treatment of transnational flows in the 1960s. This analysis delineates structure in the world system and various positions within it. The second addresses the specific issue of the effect of structural position in the system on nations' economic growth from 1955 to 1970.[3] Although these analyses are empirically independent, they converge on similar substantive conclusions concerning the operation of the world economy in the postwar period. We focus primarily on economic growth but also suggest the utility of our line of inquiry for other hypotheses suggested by exogenetic perspectives on domestic processes.

EXPLAINING DIFFERENTIAL ECONOMIC GROWTH AMONG NATIONS

Several cogent expositions of the alternative theoretical statements have recently appeared (e.g., Chase-Dunn 1975; Portes 1976; Chirot 1977; Delacroix and Ragin 1978). Therefore, we briefly summarize the relevant substantive themes and then focus more intensively on the empirical work that is of primary interest here.

Many developmental explanations treat economic growth largely as a

[3] In order to avoid terminological confusion at the outset: *Wealth* refers to the average economic welfare of a population (which we operationalize as GNP per capita) at a single point in time. *Economic growth* designates only changes over time in wealth. We use the term *economic development* (only in a few necessary instances) in its conventional, loose sense of a complex of institutional changes that typically facilitate sustained economic growth. Economic growth and economic development are not identical; the former can occur without the latter (e.g., the Spanish bullion trade). *Differential economic growth* simply identifies variations among nations on this criterion. *Position* (in its verbal world-system and formal blockmodel usages) and economic growth are separate though plausibly empirically related dimensions of international stratification. Position is a discrete variable whose values (e.g., core) are understood in relation to other values (e.g., periphery). Economic growth, wealth, etc. are continuous variables whose values (e.g., $500 per capita) have independent meaning. Perhaps the best analogue is the distinction between class and income (see Wright and Perrone 1977).

process endogenous to nations and as an ordered sequence in which currently poor countries can repeat the strategies and experiences of wealthy nations. For example, neoclassical economic formulations (e.g., Rostow 1960) stress the importance of internal mechanisms that reduce consumption and increase savings, investment, and capital formation.[4] That is not to claim that such processes operate entirely independent of exogenous influences. Economic development in poor nations is enhanced by importation of investments and by more direct forms of foreign aid (both in money and technical knowledge). What has loosely been termed "modernization theory" in sociology makes roughly similar assumptions, although its substantive focus is more on shifts in attitudes, aspirations, and values engendered by contact with modern institutions (Inkeles and Smith 1974).[5]

Developmental explanations have been criticized on a variety of general theoretical (Bendix 1967; Portes 1976) and specific empirical grounds (e.g., Landes 1969, pp. 77–80).[6] More important in generating alternative formulations, however, is simply the prima facie evidence: Despite decades (in some cases, centuries) of cultural contact, foreign investment, etc., there has been relatively little change in international economic stratification. With few exceptions, poverty is endemic in the Third World while rich nations continue to increment their wealth more rapidly (Horowitz 1972; Portes 1976, pp. 56–59).[7]

World-system/dependency theories (e.g., Wallerstein 1974; Chirot 1977; Galtung 1971) attempt to explain those phenomena and are characterized by the following major themes. First, the world system is the appropriate point of conceptual orientation. The behavior and experiences of its constituent geopolitical units depend fundamentally on features of the system

[4] According to Rostow, an investment rate of roughly 10%–12% of net national product is required for the "takeoff" into "self-sustained" economic growth. He also develops a parallel but secondary model of intersectoral linkages, which we ignore here.

[5] There are differences between evolutionary theories such as Rostow's and theories emphasizing development of values (see Portes 1976). We combine them here because of similar assumptions concerning economic growth as a largely endogenous process and the positive effects of international contact that does occur. We also acknowledge that some theories subsumed within the developmental approach (Weber on religious values and capitalism is probably most widely known) do not make all the assumptions we have attributed to this line of argument. Although our characterization of developmental theories is therefore a restricted one, it pertains to most arguments that inform current analyses of economic growth.

[6] For example, the general criticisms typically involve arguments that structural changes over time (e.g., in the scale of enterprise) render it impossible for poor nations to repeat the historical experience of rich ones. Among a variety of empirical points, Landes cites evidence that early capital formation in Britain and France most likely never approached Rostow's "takeoff" level.

[7] Given strict assumptions of unilinear processes of economic growth, there may be no reason to expect shifts in countries' relative wealth. However, the positive effects of international economic and value transmission predicted by most current developmental approaches imply that some reordering should occur as a function of those factors.

as a whole (e.g., a capitalist world economy) which reflect transnational linkages (Wallerstein 1974). Second, the modern world system is composed of three structural positions: core, semiperiphery, and periphery (see Wallerstein [1974, pp. 301–2, 349–50] and Chirot [1977, p. 13] for defining characteristics of each).[8] Third, these labels are not merely descriptive. They indicate an international division of labor in which the core is linked to the periphery (and semiperiphery) in dynamic and exploitative ways. Chase-Dunn (1975), Delacroix (1977), and Portes (1976) summarize several mechanisms that in principle reinforce structural barriers against economic growth in peripheral countries. For example, net resource flows are lost to the core as returns on investments. Additionally, peripheral economies are poorly integrated because of production that is both externally oriented and specialized in raw-material commodities. Other arguments deal not with economic development per se, but with increased inequality within nations as a function of national elite control of the export economy and alliances with core powers (Chase-Dunn 1975; Rubinson 1976). Finally, this world system is currently a capitalist world economy (Chirot 1977) and has been for several centuries (Wallerstein 1974). While the bases of this conclusion are obvious from the foregoing discussion, it is useful to recognize (as Chirot and Wallerstein do) that alternative forms of the world system are possible.

Empirical evidence concerning the development of the modern world system is fragmented and descriptive because long-run historical data are typically unavailable (Wallerstein 1974, p. 8). However, several efforts address the effects of indicators of international dependency on economic growth in the post–World War II period. More complete data for this recent period permit using rigorous analytic methods to test hypotheses derived from world-system/dependency theory. For example, Chase-Dunn (1975) uses a sample of poor countries and a panel regression model of change in GNP per capita from 1950 to 1970 to examine neoclassical and dependency models of the impact of foreign investment on economic growth and inequality. His most important finding is that the influence of domestic investment (capital formation) is positive and that of foreign investment negative. This result supports the dependency argument that it is not capital per se but the institutional locus of capital that is central. Chase-Dunn also finds expected effects of dependency measures on inequality within nations, as does Rubinson (1976) in a related but more elaborate analysis.[9] Although neither study

[8] Chirot (1974, pp. 179–81) maintains that the semiperiphery/periphery distinction may have less validity and utility in the post–World War II period than previously, because almost all noncore nations are now semiperipheral. While we recognize that possibility, we retain the conventional definitions of core, semiperiphery, and periphery pending any changes necessitated by our analysis of structure and position in the world system.

[9] We are not substantively concerned with intranational inequality in this paper. However, these studies are relevant here insofar as their independent variables attempt to capture dimensions of the world economy.

can directly represent the hypothesized intervening mechanisms, they provide clear empirical support for world-system/dependency arguments in the postwar period.

This is not to claim that all empirical results in these studies are consistent with world-system hypotheses. Delacroix (1977) reports that extent of specialization in export of raw materials (a measure that addresses world-system arguments of Galtung and Wallerstein) has no effect, net of initial wealth and secondary school enrollment, on economic growth. Given the poor performance of the world-system variable and the important effects of the endogenous measures, he properly cautions against dismissing ontogenetic processes from models of development. In a similar vein, Delacroix and Ragin (1978) analyze the influence of one form of cultural imperialism (film imports). They find that Westernized information flows have a negative effect on economic growth, but one that can be deflected by endogenous factors (particularly "mobilizing regimes"). Substantively, their results suggest that economic growth of nations is a joint function of exogenetic and ontogenetic variables.

THE EMPIRICAL STATUS OF WORLD-SYSTEM/DEPENDENCY THEORY: A CRITICAL APPRAISAL

We view the studies cited above as extremely important in establishing, though sometimes with qualifications, the merits of world-system/dependency explanations of econmic growth during the past few decades. Nevertheless, we propose three related criticisms concerning the empirical status and quantitative treatments of these arguments, although all points do not necessarily apply to each study.

1. There are no operational criteria for classifying countries according to core, semiperipheral, or peripheral location in the system or for assessing temporal shifts in nations' positions. It is obvious that multiple dimensions underlie the concept of position and that such positions are correlated with but by no means identical to economic development (Chirot 1977; Galtung 1971; Wallerstein 1974). However, we find no precise rules of classification, a problem that is perhaps best highlighted by Chirot's (1977) admitted difficulty in specifying where Communist bloc nations are structurally located in the postwar period. Moreover, there is no strong indication that a three-tiered model (core, semiperiphery, periphery) characterizes the operation of the system as a whole. Put another way, empirical treatments have no clear way in which to validate either the specified number of positions or the structural relations among them. These issues would be less problematic if the three-tiered model were simply a heuristic device. However, at least some world-system arguments indicate that the core-semiperiphery-periph-

ery distinctions are structurally necessary in a capitalist world economy (see Wallerstein 1974, p. 349).

2. Previous regression analyses of dependency hypotheses typically require a choice between two less than optimal research strategies. Some studies limit examination to samples of poor countries (Chase-Dunn 1975; Delacroix and Ragin 1978) because they want to address the effects of economic or cultural imperialism on economic growth. But if core and periphery are dynamically linked in theory, we would argue that empirical treatments should represent not only the "costs" of location in the periphery but also the "gains" of core position. The practical difficulty of doing so is that the substantive meaning of standard dependency measures plausibly differs by location in the world system. As Portes (1976; see also Chirot 1977) points out, countries such as Canada are highly investment dependent but they are also wealthy. The alternative choice—including all nations for which data are available in a single analysis—therefore often (but not always; we would exempt Delacroix [1977] from this criticism) necessitates dubious arguments. For example, Rubinson (1976, p. 654) maintains that greater dollar value of a nation's exports and imports (as a percentage of gross domestic product) "puts the state and its economic actors in a position of less power and control in the world-economy." That argument may hold for poor countries, but we do not find it compelling for nations such as the United States and (especially) Japan, whose core location appears to rest in part on ability to increase their trade in the world market.

3. Despite explicit claims that indicators such as investment dependence, trade concentration, etc. measure "position" or "control" in the world system (Rubinson 1976; Delacroix and Ragin 1978), that is not the case. These are continuous variables that stratify nations on given criteria, and no doubt partially reflect position.[10] But they do not represent such position any more than an individual's income or education measures his or her (discrete) class position (Wright and Perrone 1977; see also White and Breiger 1975, p. 68). Moreover, standard dependency measures do not fully specify the institutional locus of transnational flows. For example, the value of investment dependence of a given (say, peripheral) nation does indicate the degree of foreign investment in that country. However, it does not identify whether transnational capital flows originate entirely, partly, or negligibly from core versus other groups of nations. The differential effect of those origins is theoretically plausible and also promises to have considerable policy relevance. Finally, we want to stress that these arguments do not maintain that the use of continuous dependency indicators in models of the

[10] In making this argument, we follow theoretical proponents of world-system approaches (Wallerstein 1974; Chirot 1977; Galtung 1971) in conceptualizing position as a discrete location in the system and control as one possible form of structural relations among positions or actors.

determinants of economic growth is inappropriate. Trade concentration, investment dependence, etc. do address dimensions of international interaction which are at least partially independent of position as we conceptualize it. There is no necessary reason why these indicators should be less important than position or the origin-destination patterns of transactions in explaining countries' differential economic growth.[11] In fact, it is quite possible that both the magnitudes of international interactions and the positions of actors involved in them must be incorporated into analyses of economic growth.

WORLD-SYSTEM THEORY AND BLOCKMODEL APPROACHES

Taken together, the foregoing criticisms indicate a pressing need for empirical approaches that address world-system theories more closely via identification of structure and position. We have indicated that the substantive themes of the world-system/dependency perspective encompass (*a*) specification of discrete structural positions (core, etc.) in a single system; (*b*) identification of the units in each position and temporal shifts in those locations (e.g., Wallerstein's discussion of Spain's movement from core to semiperiphery); (*c*) structural relations among positions (exploitative links between core and periphery); and (*d*) the consequences of one position versus another for individual nations (e.g., accumulative advantage of core location vs. endemic poverty in the periphery). As White et al. (1976) point out, the general theme that actors' behaviors are influenced by their positions in a social structure is a venerable one and has generated various methods of representing structure and position empirically. White et al. also demonstrate several advantages of their blockmodel treatment over previous approaches to identifying social structure (see also Arabie, Boorman, and Levitt 1978). One reason why we prefer blockmodel analysis relative to alternative procedures is that it constitutes more than simply a technique (though we emphasize its technical aspects and applications here). In providing concrete statements concerning "structure," "position," "role," and relations among these constructs, blockmodel analysis contains the elements for a formal (though still very abstract) theory of social structure.

We consider that the general logic, procedures, and substantive utility of blockmodels are sufficiently well established that they need no extended discussion here (see also Breiger, Boorman, and Arabie 1975; White and

[11] As earlier indicated, there are two separate issues to be addressed: (1) a general inquiry into the form and operation of world-system structure in the post–World War II period, in which position is a necessary component; and (2) an analysis of a specific outcome (differential economic growth), in which position is merely expected on theoretical grounds to be an important determinant. Conventional dependency indicators are inappropriate only for the first of these two investigations.

Breiger 1975; Boorman and White 1976; Breiger 1976; Mullins et al. 1977). However, three central features warrant emphasis. First, the social structure of a population (arbitrarily defined) is specified by the multiple networks of interaction among members of that population. Second, positions ("blocks") in the structure are aggregates of actors who manifest similar patterns of interaction across all relevant networks. This second feature begins to differentiate blockmodel analysis from conventional (e.g., sociometric) approaches (White et al. 1976, pp. 736–37). Finally, the presence or absence of "bonds" (links on each type of network relation) among positions facilitates inferences concerning the form and operation of the social structure.[12]

These features of blockmodel approaches and the substantive questions posed by world-system/dependency theories are clearly complementary. Such methods plausibly resolve, at least in part, the problems discussed above with respect to empirical treatments of world-system structure. In fact, while blockmodel analysis has been largely applied to networks of individual actors, it has some special advantages concerning international interactions.[13] Such advantages are (1) the relative durability and nonreactivity of recorded information on interactions (e.g., compared with questionnaire data), and (2) the inclusion of almost all (depending on data availability) actors in the population of interest.

CONSTRUCTING A BLOCKMODEL OF THE WORLD SYSTEM: DATA AND SUBSTANTIVE CONSIDERATIONS

Nations are the constituent units or members of the "population" in our analysis. Beyond the practical consideration that network data for geopolitical units are typically available on an international basis, it is also the case that national boundaries sharply delimit the relevant interactions discussed below. Moreover, it is hardly controversial to specify nation states as the most important actors in the modern world system (Wallerstein 1974;

[12] Further aggregation and decomposition of these networks of bonds (analysis of "role structures" [see Boorman and White 1976]) generate higher-order inferences concerning the operation of social systems. We do not pursue these additional analyses because they (1) introduce questions concerning roles at a level of complexity not yet substantively addressed by world-system theory, and (2) are especially suited for comparing structures across space and/or time. Although we raise possibilities for longitudinal studies of the world system in our conclusions, the network data employed here pertain to system structure at a single time period.

[13] This is not to claim the absence of other network approaches to international flows. The basic substantive ideas that underlie blockmodels of the world system are stated in Haas (1964, p. 53) and Brams (1966, pp. 880–82). And in an innovative analysis Brams (1965, 1966) applies a hierarchical decomposition technique to transnational flows. However, his approach is entirely "sociometric" in its search for "cliques," neglect of isolate nations, and separate analysis of each network. It therefore differs from blockmodel treatments in several important ways (White et al. 1976, pp. 736–37).

Chirot 1977). We operationally define the structure of that system according to four types of international networks: trade flows, military interventions, diplomatic exchanges, and conjoint treaty memberships. We first present the organization, sources, and coding procedures used for each type of network data and then discuss the general substantive considerations and limitations that pertain to our operationalization.

The four networks are represented by separate $N \times N$ matrices of countries. The matrices are all coded in binary form as follows. Each "claim" or "instance of a tie from one member of a population to another" (White et al. 1976, p. 768) is designated "1" in the appropriate cell of the matrix, and the absence of a claim coded "0." For the present analysis, we limited collection of data to a period circa 1965 in order to ensure a large number of cases while retaining the diplomatic relations information (which is available at only one point in time). Our blockmodel analyses are based on 118 nations for which data were available for all networks.[14] Where possible, our coding procedures also utilized a short (and arbitrarily selected) series of years circa 1965 in an attempt to minimize idiosyncratic characteristics of a single year.[15] Details concerning construction of each matrix follow.

Trade.—Each country's yearly dollar value of exports to every other country is reported in a serial publication of the International Monetary Fund and the International Bank for Reconstruction and Development (n.d.). Import data are also published, though they virtually mirror the export figures (there are minor accounting variations). Our criterion for the presence of a "tie" or "claim" is a nonzero value of exports from country A to country B in at least two of the five years from 1963 to 1967 inclusive.[16] We also note that the data contain an underreporting bias. The figures for all "Soviet areas" (Albania, Bulgaria, Cuba, China, East Germany, Czechoslovakia, Hungary, Mongolian Republic, North Korea, North Vietnam, Poland, Rumania, and the Soviet Union) and 21 other nations in the sample are at least partially reconstructed from export and import values of reporting countries. We coded trade ties as present among Soviet area countries,

[14] There is one minor exception: the trade data only were published for Belgium and Luxembourg combined. We attributed the reported flows to each country, which probably slightly overestimates the breadth of their trade networks. However, we considered this procedure preferable to dropping both countries from the analysis.

[15] Single-year information would be particularly problematic with the interventions data. For example, given extreme hostility between countries A and B, whether A intervenes into B in 1964 or 1965 (as well as lag time for B's possible retaliation) appears highly susceptible to idiosyncratic factors in the short run.

[16] The smallest nonzero value reported is $100,000. We use the criterion of at least two years to eliminate one-time-only transactions, though very few such instances are actually found in the data. We originally coded the data in several categories, but the vast bulk of trade flows falls into the two extreme categories: no link whatsoever or else large amounts of exports in each year. Therefore, we suspect that the actual cutting point used makes little empirical difference.

which appeared more valid than assuming the absence of all such ties. Of the remaining 21, only 11 nations had reconstructed data for more than three of the five years on which our coding is based. Therefore, links among these 11 nations and between any of them and the 13 Soviet areas could not have been reported in the data source. We doubt that this bias is drastic because most of the 11 countries with entirely reconstructed data had very few trade links even with reporting nations.

Interventions.—The Taylor-Hudson (1972) World Handbook II Project coded every military intervention (land, air, or sea incursion of one nation into the territory of another) reported in the *New York Times Index* from 1948 through 1967. We obtained a copy of the individual intervention events data file (described in Inter-University Consortium for Political Research [1971, sec. IV]). Our criterion for a tie is at least one intervention of country A into country B during any of the years from 1960 through 1967 inclusive. We suspect that events of the magnitude of military interventions are nearly fully reported (see Snyder and Kelly 1977). Pearson's (1974) attempt to validate portions of these data suggests that is the case, although he finds one intervention omitted in each of the two years he surveys.

Diplomats.—Steven Brams provided an origin-destination matrix of the number of career-level diplomats in either 1963 or 1964 (exact date varies by country). These data were gathered from lists published by host nations, supplemented with questionnaire information, and are elsewhere described in more detail (Brams 1965, 1966). Our criterion for a tie is at least one diplomat sent from country A to the national capital of country B.

Treaties.—Small and Singer (1969, pp. 264–70) list all international treaties in effect during the years 1946 through 1965, with information on dates of inception and termination (if any) and signatory countries.[17] From their tabular data, we coded the presence of a tie as any conjoint partnership in a treaty by countries A and B during any portion of the years 1960 to 1965 inclusive.

While these four networks clearly do not exhaust the forms of international contact, each of them captures a substantively important dimension of transnational interaction. The centrality of economic and military relations (trade and interventions) in almost all approaches to international system structure is obvious (e.g., the work surveyed in Christopherson [1976]). More specifically, theories of imperialism typically emphasize the domination of the world economy by core powers (though they are not always so designated) and the use or threat of superior armed force as the means of ensuring economic domination. We also consider that diplomatic

[17] These treaties are limited to formal military defense agreements of three types: defense pacts, nonaggression pacts, and ententes (see Small and Singer [1969] for definitions of each, as well as coding criteria and original sources). We used all three types (which do not include general agreements of friendship) in coding our treaty matrix.

exchanges constitute a salient form of information flow in the world system. For sending nations, diplomatic missions provide regular and ostensibly reliable information concerning local economic opportunities, political conditions, etc. in the host country.[18] Such missions may also facilitate trade agreements and sometimes serve as a base for attempts to manipulate local conditions. We attach much less significance to the receiving of diplomats. Whatever information is transmitted about the sending country is both geographically removed from the occurrence of events and "filtered" through representatives of that country. As such, we suspect that such information is actually and perceptually less useful to host nations. Finally, we included the treaty data on the rationale that they would address networks of defense commitments or reflect attempts by some nations (e.g., core powers) to legitimate potential military intervention in others (peripheral countries).

We recognize that operationalizing our approach to structure in the world system via these four networks may be controversial. For example, why employ the diplomat and treaty networks, given the clear precedence of economic and military relations in some influential treatments (e.g., Wallerstein 1974) of world-system structure? Why not substitute types of commodities exchanged, which plausibly capture better the core-periphery division of labor, for the trade network? Our decisions on these issues were based on several conceptual and practical considerations. The most important of these concerns the relationship between blockmodel analysis and world-system theory. Blockmodel analysis is a formal network approach that generalizes across populations and particular substantive issues. In principle, investigators would employ all networks of interaction among members of a population in such analyses, particularly since no given network necessarily has any precedence over any other in the blockmodel procedures. In practice, of course, some selection of interactions is required because of resource and data availability constraints. Therefore, investigators attempt to represent "important" dimensions insofar as they (1) provide the most independent information, and (2) facilitate interpretation of the network patterns of interaction among positions in the blockmodel. For example, blockmodel studies typically include networks of both cooperative (positive affect in the case of interpersonal ties) and conflict (antagonistic) interactions.

The world-system perspective is, among other things, a particular theory of the form and operation of international-system structure (see Christopherson [1976] for alternative models). Although each of our four networks could be conceptualized in terms of core-periphery exploitative dynamics (see the discussion above), it is not necessary to do so. In fact, it would be inappropriate if the generalized approach to social structure

18 Wallerstein (1974, p. 22) cites an interesting medieval parallel to this argument. In addition, the empirical results we present subsequently provide at least a partial validation of our reasoning concerning diplomatic relations.

(blockmodel analysis) were based solely upon networks specified by a variant (which stresses economic relations, such as Wallerstein [1974] and Chirot [1977]) of a single theory of that structure. Many alternative models of international-system structure also emphasize the importance of noneconomic transactions (e.g., Brams 1966; Christopherson 1976). Moreover, even some analyses that view the world system in essentially core-periphery terms (Galtung 1971) identify system structure as a joint function of economic, military, political, and cultural relations.

While this specification of the relationship between blockmodel analysis and world-system theory justifies the inclusion of noneconomic dimensions such as diplomatic exchanges and treaties, it does not constitute a warrant for excluding more detailed economic transaction data (e.g., types of commodities exchanged, investment capital flows). In this case, data availability influenced our strategic decisions. For the mid-1960s period, information on commodities and other types of economic interactions can be constructed only for a substantially attenuated set of nations (relative to the 118 in the present analysis). We could have employed more recent commodity data, which have greatly expanded coverage. However, information on interventions, diplomats, and treaties is not yet available for the 1970s. We have already indicated the undesirability of deleting such networks. Therefore, we chose to represent multiple dimensions of interaction for nearly the universe of nations, even though our network of economic relations would be a crude one. Moreover, we wanted to avoid the possibility that certain results (e.g., a core-periphery structure) might be based only on the economic indicators most likely to produce them.

This is not to claim that our operationalization is optimal. We invite replications with additional or different networks as they become available. We also recognize that different results could be obtained (though that becomes less likely as more networks add successively less independent information). While such issues are far from trivial, they are subsidiary to our main purpose of demonstrating the potential power of blockmodel approaches to the substantive questions posed by world-system theory.

Two further issues require brief mention preliminary to the presentation of our blockmodel findings. First, our empirical analysis necessarily specifies a single world system that encompasses all nations, although it may identify "isolates" as a unique structural position (see White et al. 1976). Some areas may lie entirely outside the world economy, especially historically (see Wallerstein 1974). However, the fact that all nations had ties to other countries in the 1960s suggests that the assumption of a single world economy during the recent period covered by our data is largely warranted. Put another way, if there are a few exceptions (e.g., stateless societies not included in our sample), they should have no material bearing on our analysis. Second, we want to emphasize that our results bear upon the world system

only at a single point in time. They are not intended to address either the historical development of that system (though knowledge of past occurrences can inform our interpretation) or the mechanisms internal to nation states on which we lack direct measures.[19]

STRUCTURE AND POSITION IN THE WORLD SYSTEM: BLOCKMODEL RESULTS AND DISCUSSION

We applied CONCOR (see Breiger et al. 1975; White et al. 1976) to the matrices described above.[20] Figure 1 displays the number of countries in each successive partition of the data, with blocks designated by arbitrary letter "names." Here we deal only with the 10-block partition of the data (those sets of countries labeled A through F′). While the 10-block split is necessarily consistent with four- and six-block partitions, it presents a more refined picture of the overall structure and also generates the greatest ex-

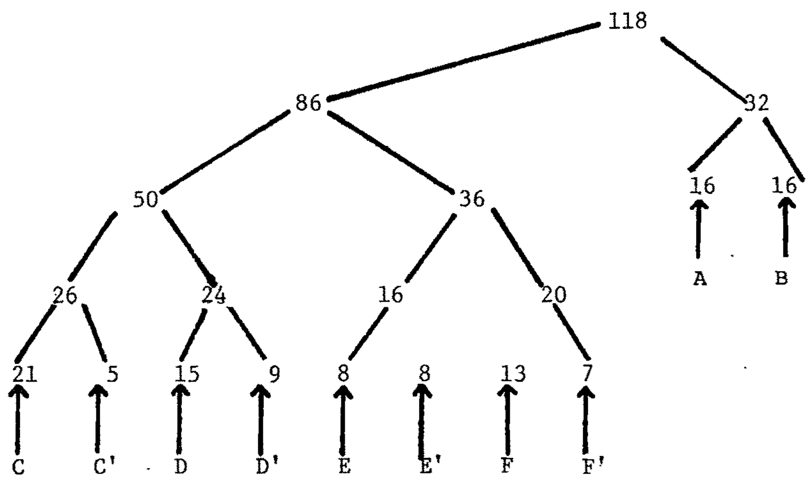

FIG. 1.—Blockmodel "tree" of hierarchical decompositions: 118 nations circa 1965. Letters designate block "names" in 10-block model. See table 1 for list of countries located in each block.

[19] By stating these limitations (which also apply to other empirical analyses of world-system issues), we want to avoid metatheoretical criticisms of the form advanced by Bach (1977) and Irwin (1977). We grant the validity of some of their points. However, they apply more to analyses of issues (e.g., intranational inequality) in which unmeasured domestic attributes such as class structure are central, and which lack time-ordered data. These points are not directly relevant at the level of analysis on which we are operating in this paper

[20] CONCOR involves weaker assumptions than the alternative BLOCKER algorithm discussed by White et al. (1976). We employed CONCOR because this first application of blockmodels to world-system structure is exploratory. We also note that studies which employ both algorithms report highly consistent partitions of the data.

planatory power in our subsequent empirical analyses. Table 1 lists the countries in each block for the reader's convenience. However, most of our interpretation does not rely directly on this information.

Table 2 presents the set of binary-image matrices that indicates the presence or absence of "bonds" among blocks on each type of network. The image matrices reflect in more simplified form the patterns of ties among blocks depicted in the density matrices of table 3. With the exception of the treaty matrix (wihch is symmetric by definition), the rows for each dimension indicate the "sending" blocks and the columns the "receiving" ones. For example, the fifth entry in the first row of the trade-density matrix (.744) in table 3 indicates the proportion of actual relative to possible export links from block C nations to block E nations. The binary entries in the image matrices reflect whether each density displayed in table 3 is greater (coded "1") or less (coded "0") than the mean density for that matrix. Although this criterion is of course arbitrary, the overall patterns described below are not highly sensitive to the particular cutting point chosen.

In general, though with some deviations, we interpret the pattern of bonds depicted in the image matrices as a core-semiperiphery-periphery structure

TABLE 1

LISTING OF COUNTRIES IN EACH BLOCK: FOUR-NETWORK BLOCKMODEL (Trade, Interventions, Diplomats, and Treaties) FOR 118 NATIONS CIRCA 1965

Block	Nations
A........	Chad, Congo (Brazzaville), Congo (Kinshasa), Uganda, Burundi, Rwanda, Somalia, Ethiopia, Malagasy Republic, Morocco, Algeria, Tunisia, Libya, Sudan, United Arab Republic,* Yemen*
B........	Mali,* Mauritania,* Ghana,* Upper Volta,* Senegal, Dahomey, Niger, Ivory Coast, Republic of Guinea, Liberia, Sierra Leone, Togo, Cameroun, Nigeria, Gabon, Central African Republic
C........	Canada, United States, United Kingdom, Netherlands, Belgium, Luxembourg, France, Switzerland, Spain, Portugal, West Germany, Austria, Italy, Yugoslavia, Greece, Sweden, Norway, Denmark, South Africa, Japan, Australia
C′........	Venezuela, Peru, Argentina, Uruguay, South Korea
D........	Cuba, Ireland, East Germany, Hungary, Cyprus, Bulgaria, Rumania, USSR, Kenya, Iran, Turkey, Iraq, Lebanon, Jordan, Israel
D′........	Finland, Saudi Arabia, Taiwan, India, Pakistan, Burma, Ceylon, Malaysia, Philippines
E........	Panama, Colombia, Ecuador, Brazil, Bolivia, Paraguay, Chile, North Vietnam
E′........	Haiti, Dominican Republic, Mexico, Guatemala, Honduras, El Salvador, Nicaragua, Costa Rica
F........	Jamaica, Trinidad and Tobago, Poland, Czechoslovakia, Malta, China (People's Republic), Mongolian Republic, Nepal, Thailand, Cambodia, Laos, New Zealand, Iceland
F′........	Albania, Syria, Kuwait, Afghanistan, North Korea, South Vietnam, Indonesia

* Starred countries in blocks A and B cluster together in further "splits" of the data, although they are not shown as separate blocks in the analyses.

TABLE 2 BINARY "IMAGES" OF TEN-BLOCK MODEL .

TRADE

	C	C'	D	D'	E	E'	F	F'	A	B
C	1	1	1	1	1	1	1	1	1	1
C'	1	1	0	1	0	1	0	0	0	0
D	1	0	1	1	0	0	0	0	0	0
D'	1	1	1	1	0	0	0	1	0	0
E	1	0	0	0	1	0	0	0	0	0
E'	1	1	0	0	0	0	0	0	0	0
F	1	0	0	0	0	0	0	0	0	0
F'	1	0	0	0	0	0	0	0	0	0
A	1	0	0	0	0	0	0	0	0	0
B	1	0	0	0	0	0	0	0	0	0

INTERVENTIONS

	C	C'	D	D'	E	E'	F	F'	A	B
C	0	0	1	0	1	0	1	0	1	1
C'	0	0	0	0	1	1	0	1	0	0
D	0	1	1	0	1	1	0	0	1	0
D'	0	0	0	1	0	0	1	0	0	0
E	0	1	1	0	1	0	0	0	0	0
E'	0	0	0	0	1	1	1	0	0	0
F	0	0	0	0	0	0	1	0	0	0
F'	0	0	1	1	0	0	1	0	0	0
A	0	0	0	1	0	0	0	0	1	1
B	0	0	0	0	0	0	0	0	0	0

TABLE 2 (Continued)

DIPLOMATS

	C	C'	D	D'	E	E'	F	F'	A	B
C	1	0	1	1	1	1	1	1	1	1
C'	0	0	0	0	0	0	1	0	0	0
D	0	0	1	1	1	0	1	1	0	0
D'	0	0	1	0	1	0	1	1	0	1
E	0	0	0	0	1	1	1	1	0	0
E'	0	0	0	0	1	1	1	1	1	0
F	0	0	0	0	0	0	1	0	0	0
F'	0	0	0	0	0	0	1	0	0	0
A	0	0	0	0	0	0	1	0	0	0
B	0	0	0	0	0	0	1	0	0	0

TREATIES

	C	C'	D	D'	E	E'	F	F'	A	B
C	1	0	0	0	0	0	0	0	0	0
C'	0	1	0	0	1	1	0	0	0	0
D	0	0	1	0	0	0	0	1	0	0
D'	0	0	0	0	0	0	0	0	0	0
E	0	1	0	0	1	1	0	0	0	0
E'	0	1	0	0	1	1	0	0	0	0
F	0	0	0	0	0	0	0	0	0	0
F'	0	0	1	0	0	0	0	0	0	0
A	0	0	0	0	0	0	0	0	1	1
B	0	0	0	0	0	0	0	0	1	1

TABLE 3 TEN-BLOCK DENSITIES

TRADE (Mean Density = .3410)

	C	C'	D	D'	E	E'	F	F'	A	B
C	.943	.895	.908	.889	.744	.714	.630	.612	.622	.500
C'	.829	.560	.320	.422	.250	.525	.215	.114	.087	.100
D	.854	.267	.613	.474	.033	.133	.282	.286	.271	.108
D'	.884	.533	.459	.617	.153	.194	.308	.349	.215	.069
E	.679	.225	.042	.139	.422	.172	.106	0	.023	0
E'	.643	.525	.125	.125	.125	.297	.135	.036	.039	.023
F	.634	.215	.262	.265	.029	.135	.183	.165	.144	.087
F'	.551	.171	.229	.317	.018	.018	.132	.082	.045	.009
A	.604	.037	.225	.181	0	.039	.115	.045	.141	.063
B	.533	.025	.092	.042	0	.008	.038	0	.070	.160

INTERVENTIONS (Mean Density = .0072)

	C	C'	D	D'	E	E'	F	F'	A	B
C	0	0	.016	0	.042	.006	.015	.007	.030	.030
C'	0	0	0	0	.025	.025	0	.029	0	0
D	0	.013	.013	0	.008	.008	.005	0	.017	.004
D'	0	0	0	.025	0	0	.017	0	.007	.007
E	0	.025	.008	0	.047	0	0	0	0	0
E'	0	0	0	0	.031	.016	.019	0	0	0
F	0	0	0	0	0	0	.018	0	.005	0
F'	0	0	.019	.016	0	0	.011	0	0	0
A	0	0	.004	.014	0	0	0	0	.070	.008
B	0	0	0	0	0	0	0	0	0	0

DIPLOMATS (Mean Density = .2644)

	C	C'	D	D'	E	E'	F	F'	A	B
C	.413	.200	.533	.434	.542	.548	.872	.626	.440	.342
C'	.133	0	.067	.044	.200	.225	.400	.257	.138	.075
D	.238	.040	.347	.304	.325	.242	.759	.314	.221	.246
D'	.143	0	.311	.259	.417	.222	.684	.429	.243	.285
E	.173	0	.108	.042	.266	.391	.442	.339	.203	.086
E'	.202	0	.175	.097	.297	.359	.635	.411	.313	.172
F	.117	0	.185	.188	.202	.163	.527	.132	.111	.139
F'	.136	0	.257	.175	.196	.196	.518	.163	.134	.161
A	.119	.050	.154	.118	.133	.133	.447	.143	.129	.133
B	.092	.075	.146	.104	.078	.141	.452	.098	.109	.141

TABLE 3 (Continued)

TREATIES (Mean Density = .1278)

	C	C'	D	D'	E	E'	F	F'	A	B
C	.324	.048	.044	.053	.048	.042	.073	0	.003	0
C'	.048	.480	0	0	.800	.700	0	0	0	0
D	.044	0	.142	.052	0	0	.067	.152	.092	0
D'	.053	0	.052	.025	0	0	.043	.032	.049	0
E	.048	.800	0	0	.875	.875	0	0	0	0
E'	.042	.700	0	0	.875	.656	0	0	0	0
F	.073	0	.067	.043	0	0	.024	.044	0	.010
F'	0	0	.152	.032	0	0	.044	.041	.125	0
A	.003	0	.092	.049	0	0	0	.125	.867	.938
B	0	0	0	0	0	0	.010	0	.938	.938

in which (1) block C constitutes the core; (2) blocks E through B (in the order shown) are the periphery; and (3) block D, and perhaps also C′ and D′, are located in the semiperiphery of the world system.[21] This three-tiered structure is most clearly reflected in the image matrix for trade (see White et al. [1976] for structurally similar patterns with different data). Block C is unambiguously at the core of the world economy. It maintains trade linkages to and from every other block in the system. The density matrix in table 3 similarly indicates that every block has more trade linkages with C than with any other block.[22] At the other extreme, blocks E through B are with minor exceptions integrated into the world economy only through their trade with the core block C. This pattern indicates a peripheral location in the overall structure. Blocks C′, D, and D′ differ from both the "core" and "periphery" (as we designate them). There are mutual trade links among and within these blocks, and these blocks are therefore partially independent of the core block in the world economy. However, blocks C′, D, and D′ are for the most part not directly linked to the peripheral blocks. In addition,

21 We stress that the identification of blocks and inferences concerning system structure are based on a single (simultaneous) partitioning of all four interaction networks. Our discussion below of each network separately is intended only to convey information concerning variations in patterns across types of interaction; it does not imply that each network constitutes a separate blockmodel.

22 CONCOR's dichotomous splits of the raw data (Schwartz 1977) and investigators' decisions of how many partitions to undertake necessarily result in arbitrarily determined numbers of blocks. Therefore, the location of countries in blocks is by no means definitive, and we are not wedded to the precise placement shown in table 1. For example, we could have further partitioned the core block of 21 nations, which would result in a smaller set of "core" nations and a larger set of "semiperipheral" countries. These further refinements do not materially affect our conclusions. For instance, deleting from the core all nations which are arguably semiperipheral would still leave very large differences between the core-periphery and semiperiphery-periphery trade densities depicted in table 3.

they are most "salient" to the core as trade partners (note the discontinuity in the densities after the fourth entry of the first row and col. in table 3). Structurally, blocks C′, D, and D′ reflect a "hangers-on" pattern (White et al. 1976) rather than being part of a simple hierarchy.[23] Substantively, we are therefore inclined to label blocks C′, D, and D′ as the semiperiphery, at least pending examination of the other networks.

The image matrix for interventions is largely consistent with conclusions based upon trade bonds. Although block C does not undertake military interventions into all other blocks, it polices much of the system (note in the density matrix that two of the zero images marginally fail to reach the criterion for a bond). Additionally, block C is the only one that receives no interventions (col. 1). Moreover, the density matrix demonstrates that these zero images are all "true" zeros, and thus entirely independent of the cutting point. No core nations experienced a single territorial incursion during the 1960–67 period covered by these data.[24] Block D is also a ubiquitous intervener, though it differs from C because it is also a frequent target of other blocks (including C). The less global intervention patterns of blocks C′ and D′ reduce our confidence in placing them in a semiperiphery. The remaining blocks vary a good deal in their intervention patterns. In general, there is somewhat more of a hierarchical structure in the intervention than in the trade images. For example, block A countries are intervened into by different, and apparently more powerful, blocks (C and D) than those into which they intervene (D′ and B; block A nations' interventions into each other also manifest the highest density by far in the entire matrix). Similar patterns characterize blocks C′, E′, F, and B. However, indications of any overall hierarchy in the system (except for the military dominance of C) are not strong.

The diplomatic exchange bonds are consistent with our previous hypothesis concerning the differential importance of sending and receiving missions. Block C clearly dominates the sending of these information flows in the system, but it does not receive diplomatic bonds from any other block (col. 1 of the image matrix). Block F is the main receiver, although the meaning of that is ambiguous. Given that block F is also the frequent target of interventions from other blocks, we originally suspected that receiving diplomats might reflect global "crisis locations." But a comparison of the interventions and diplomats matrices indicates no strong support for that contention. As in the interventions matrix, there are modest indications of a hierarchy in

[23] These three blocks do not manifest a clearly dominant pattern of trade over those that lie in the periphery. For example, block B's trade densities are greater among its constituent nations than with any other block except the core.

[24] This particular pattern would not of course be observed during some other periods (e.g., world wars). Multinational wars involving core nations plausibly constitute a primary source of reordering the nations in various positions, if not of altering the positions themselves.

which each location (core, semiperiphery, periphery) initiates but does not receive diplomatic flows from those below it in the status ordering.[25]

In summary, the structure defined by the blockmodel analysis is generally consistent with the core-semiperiphery-periphery model specified by world-system treatments. Block C constitutes the core of the system in each type of network. Only block D is clearly located in the semiperiphery, because it alone reflects the pattern of flows (though to a much weaker extent) that characterizes block C on all interaction dimensions. While blocks C′ and D′ have trade bonds similar to those of block D, their patterns of interventions and diplomatic exchanges are more in line with those of peripheral blocks. Therefore, the placement of blocks C′ and D′ in the semiperiphery is more questionable. Finally, blocks E, E′, F, F′, A, and B clearly comprise the periphery, although there are of course differences among them.

Indeed, there must be differences between all of the blocks because in strict terms the analytic technique has identified 10 separate structural positions (blocks) in the system. Although the exact number of blocks is arbitrary, how can that consideration be reconciled with our claims of a three-tiered model? In our view, the structural relations of blocks E through B are similar enough that they override the observed variations and warrant the "periphery" designation we have attributed to them. However, we also consider that differences among these blocks reflect varying characteristics of different parts of the periphery that may also be substantively important. Such differences plausibly reflect variations in historical experiences, state strategies, etc. Space limitations preclude discussion of these variations here. However, we stress their existence because they suggest that (*a*) national (or international) experiences are not solely determined by world-system position, and (*b*) methods other than those employed here (e.g., case studies, historical analyses) are necessary to explore national variations within particular positions in the system.

THE EFFECTS OF STRUCTURAL POSITION ON THE ECONOMIC GROWTH OF NATIONS, 1955–70

The blockmodel analysis bears only on the form and operation of the world system at a given point in time. Here we integrate the blockmodel findings with our substantive focus on the economic growth of nations. Simply stated, we want to answer two questions: does block location (position) make a difference and, if so, how much of a difference in countries' economic welfare? Therefore, we report multivariate analyses of the effects of block loca-

25 We ignore the treaty matrix in our discussion because there is not much of substantive importance to be learned from it. Examination of the treaty links among individual nations in the raw-data matrix (not shown here) indicates that the bonds shown in tables 2 and 3 almost entirely reflect regionally based pacts.

tion on economic growth. These analyses constitute strong and independent (of the blockmodel results) tests of world-system/dependency arguments and of our interpretation of the blocks as structural positions.

Following Wallerstein, Chirot, and related arguments, some obvious hypotheses concerning the effects of block location on differential growth may be briefly specified. Block C, net of other factors, should experience the greatest economic growth in the system. By dominating the international trade network, it can largely determine the terms of trade with all other blocks except C′, D, and D′. Given its intervention pattern, block C can maintain its economic interests with the frequent use (and perhaps threat) of military force. Moreover, its apparent invulnerability to such intervention also favors economic growth. And because of its centrality in the diplomatic (information-flow) network, block C should achieve further advantages on the grounds discussed earlier. We expect that block D should experience, again ceteris paribus, the next greatest economic growth. Its trade network is somewhat independent of block C, and D is roughly "second" in initiating interventions and diplomatic missions. Block D should resemble block C in the consequences of those patterns, though to a considerably lesser extent. In addition, since D is a frequent target of outside military intervention, its nations' economic growth may be expected to suffer. Finally, the remaining blocks should, according to dependency arguments, manifest the largest decrements as a function of their structural position. Although we expect variations among the peripheral blocks, we are unable to specify their relative magnitudes. For example, blocks A and B are structurally similar on all except the intervention networks. Block A's greater initiation of military incursions, particularly its domination over B, might suggest an economic advantage. But A is also more often the target of interventions, which would suggest economic dislocations. The importance of these differences for economic growth cannot be readily ascertained a priori.

Our empirical analysis employs the change in gross national product (GNP) per capita from 1955 to 1970 as the dependent variable. This interval is long enough to observe substantial shifts and also increments the sample size considerably over an earlier (1950) starting point. Change in GNP/capita is regressed on initial (1955) GNP/capita (to control for its correlation with gain score: see Bohrnstedt [1969]) and on other independent variables of substantive interest. Although a panel or lagged dependent variable model is more typically employed in such analyses (Chase-Dunn 1975; Delacroix 1977), the two forms are equivalent in the parameters of primary interest (Treiman and Terrell 1975, p. 184n).[26] We use the change

[26] Metric partial regression coefficients and standard errors are identical, while the coefficient of the initial GNP/capita variable differs by a constant of 1.0. It should also be noted that the R^2 is generally considerably lower with the change dependent variable, because of the deletion of the autoregression coefficient which usually predominates empirically in the panel model.

score to emphasize our focus on economic growth (see Delacroix and Ragin [1978] for a similar treatment).

We begin with a basic model of change in GNP/capita which follows that of Delacroix (1977). It contains the 1955 GNP/capita and adjusted secondary school enrollment ratio (percentage of the secondary school age population enrolled in secondary education) as explanatory variables. The GNP/capita is the most conventional indicator of average economic welfare.[27] The secondary school ratio more likely influences changes in wealth than primary enrollment over the relatively short period covered by these data. It also minimizes comparability and multicollinearity problems relative to university-level enrollment figures (Delacroix 1977). Equation (1) in table 4 presents estimates of this basic model for the 90 countries for which all data are available. Nations higher in initial wealth experience greater increases over the 15-year period (we assume that this effect reflects a number of unspecified cultural and institutional factors).[28] The impact of secondary enrollment is consistent with Delacroix's findings. Each percentage point increase in 1955 enrollment generates an estimated net $21 gain in GNP/capita.

Equation (2) adds a series of nine dummy variables to the basic model, each representing block location (countries were coded "1" if they were located in the given block and "0" otherwise). Note that one category must be omitted because it is a perfect linear combination of the other dummy variables in the model. We arbitrarily omitted block C. Therefore, the coefficients of location in the other blocks are properly interpreted as the net effects of being in a given block relative to location in the omitted category or "core" block.[29] The results presented in equation (2) are noteworthy in

[27] Data for this portion of the analysis are based on a merged file of information from Taylor and Hudson (1972) and the International Bank for Reconstruction and Development (1971, 1973). The GNP and other measures taken from the latter source are identical to those employed by Chase-Dunn (1975) and Delacroix (1977). Delacroix (1977, p. 800n) provides more detailed information and proper cautions regarding the GNP data. We also recognize that correlated error in the 1955 and 1970 GNP indicators will upwardly bias the slope of initial GNP/capita. But we are not overly concerned with that bias because coefficients of the variables of substantive interest will therefore be conservative estimates.

[28] If dependency arguments are largely correct, then location of course affects initial GNP/capita. Moreover, the persisting influence over time of world-system position is at least partially embedded in the estimated autoregression coefficient. Therefore, assessments of the importance of exogenetic factors suggested by our analyses are almost certainly very conservative. We also want to acknowledge that the longer-run relationship between position and economic growth is reciprocal. Greater or lesser economic growth determines a nation's ability to change or maintain position in the system. However, the substantive questions at issue here and the logic of our research design limit our inquiry to the short-run influence of position on economic growth.

[29] We could of course calculate the effect of the omitted category (minus the weighted average of the coefficients of the included ones) and then estimate deviations from the grand mean rather than from block C. However, that procedure is not important for our current purposes.

TABLE 4

ESTIMATES OF CHANGES IN GNP/CAPITA (1955–70) REGRESSED ON CONTROL VARIABLES AND DUMMY VARIABLES REPRESENTING "STRUCTURAL POSITION" (Location in 10-Block Model)

INDEPENDENT VARIABLES[a]	PARTIAL REGRESSION COEFFICIENTS (Standard Errors)			
	Eq. (1)	Eq. (2)	Eq. (3)	Eq. (4)
GNP/capita (1955)	.292 (.080)**	.274 (.081)**	.266 (.082)**	.294 (.082)**
Secondary school enrollment ratio (1955)	20.99 (2.37)**	13.62 (2.95)**	16.98 (2.76)**	11.18 (3.15)**
Block A	...	−468.0 (170.4)**	...	−329.0 (163.1)*
Block B	...	−507.4 (184.7)**	...	−449.6 (182.1)*
Block C′	...	−645.4 (186.0)**	...	−626.2 (184.1)**
Block D	...	−422.7 (139.7)**	...	−386.6 (146.3)**
Block D′	...	−510.0 (180.1)**	...	−490.5 (179.9)**
Block E	...	−542.8 (202.6)**	...	−504.3 (197.9)*
Block E′	...	−582.3 (177.5)**	...	−546.5 (175.9)**
Block F	...	−310.0 (153.4)*	...	−257.3 (158.9)
Block F′	...	−789.5 (204.3)**	...	−758.3 (204.7)**
Diplomats sent (1963–64)	...	...	.142 (.297)	.433 (.309)
Interventions into (1960–67)	...	...	−1.03 (3.54)	−3.44 (3.59)
Interventions by (1960–67)	...	...	−11.47 (4.42)*	−12.68 (4.49)**
Value of exports (1965; millions of U.S. $)	...	...	.036 (.018)*	.010 (.019)
Regression constant	20.33	562.2	51.1	507.6
R^2	68.1%	75.9%	72.7%	79.1%

NOTE.—N = 90 nations.
[a] Dependent variable is change in GNP/capita, 1955–70.
* Significant at .05.
** Significant at .01.

several respects. The coefficients of the block dummy variables are all in the negative direction expected from a core-periphery model.[30] Moreover, they are enormous: the "cost" of location in a peripheral or semiperipheral block (relative to the core) is roughly $500 per capita over this 15-year span.[31] To be sure, block location is correlated with a country's initial wealth, but these estimates are net of GNP/capita in 1955. It should be stressed that there is no necessary connection between the image and density matrices in the blockmodel analysis and the magnitudes or signs of the effects of the block dummy variables in the regression model. The convergent evidence from these two analyses therefore provides strong support for world-system/dependency theory of system structure and of the most widely investigated consequence of that structure (nations' differential economic growth). We also note that the inclusion of the block variables substantially attenuates the effects of the secondary school enrollment indicator in the "endogenous" model of economic growth. Finally, the series of dummy variables modestly increments the R^2 over the baseline model's explanatory power. This increment is perhaps more compelling in light of Delacroix's (1977, p. 805) conclusion that "in the short run, wealth leads to wealth. It is difficult to show the effect of anything on national wealth."

Despite the strength of the results presented in equation (2), they might still be artifactual. For example, core nations initiate more interventions, diplomatic missions, and trade flows and experience no military incursions. Other countries also vary along these dimensions. Therefore, it is plausible that the effects of block membership and its substantive analogue (structural position in the world system) simply reflect countries' differential values on these dimensions of international interaction. To investigate that possibility, equation (3) temporarily deletes the block dummy variables and adds the total number of interventions initiated and received by each nation (1960–67), diplomats sent (1963 or 1964), and dollar value (in millions) of exports in 1965. We are essentially (but not precisely) including the row and column marginals of the raw data matrices initially used to define the partitioning of nations into blocks. The results indicate that nations which

30 However, expected differences in the economic costs experienced by nations in semiperipheral versus peripheral locations are not supported by these results. While block D (the clearly semiperipheral block) has the second lowest decrement, the general pattern of effects does not sustain that interpretation.

31 Since our data pertain to the quasi universe of nations, standard errors and significance tests are reported solely for reasons of convention and convenience. We are quite willing to rest our conclusions on the magnitudes of the partial regression coefficients. One reader suggested that heteroscedasticity of the disturbances, which is plausible with a skewed dependent variable such as GNP/capita, might in any case bias the reported significance estimates. The regression coefficients that are of primary interest would be unbiased even if the disturbances were not homoscedastic. However, we employed a test for heteroscedasticity developed by Goldfeld and Quandt (1965; see Dutta [1975, pp. 132–36] for details on procedures) and found no evidence of it.

initiate more trade and diplomat flows experience positive economic returns (though small ones for diplomats) on those activities.[32] But the findings for interventions are surprising. The effect of receiving interventions is negligible. Conversely, the impact of interventions by a country is both substantial and negative. Moreover, this effect is "real." Other analyses (not reported in tabular form) indicate that it persists through a variety of estimating equations and is not masking different slopes across blocks (e.g., positive effects of interventions by core nations). Although we are primarily interested in the measures in equation (3) as control variables, this finding warrants brief discussion. We still maintain that nations' bonds in the intervention matrix (e.g., the dominance of block C) contribute to the effects of the block dummy variables in equation (2). However, we believe that the negative effect of the number of interventions by a country in equation (3) could reflect either of two (not necessarily exclusive) phenomena. Interventions may be initiated precisely because the target country is not under firm control, that is, as a response to declines in economic returns extracted from that country. It is also possible that this result indicates a "free rider" problem (e.g., Olson 1965). For example, while the core block may derive collective economic benefits from military imperialism, the particular nations that initiate interventions bear the direct costs of providing or ensuring that collective good.

Finally, equation (4) in table 4 adds the block dummy variables to the controls included in equation (3). Although the effects of block location are slightly attenuated in this expanded model, the absolute and relative magnitudes of the coefficients are similar enough to those of equation (2) that our substantive conclusions need not be materially altered. The effects we attribute to structural position in the system are clearly not due simply to the gross outflow and inflow (i.e., marginal) characteristics of nations' interactions in that system. Therefore, the contention that such effects must be due to the networks of interaction that define the system and position in it becomes even more plausible.

FURTHER ANALYSIS

In order to investigate as exhaustively as possible with available data whether the effects of block location are spurious, we undertook several fur-

[32] Total value of imports is correlated over .99 with exports, so we dropped it from the analysis. Therefore, the exports measure more properly reflects total trade volume. We also originally included the numbers of diplomats received and treaty ties for each nation. However, there was no clear substantive rationale for expecting effects on economic growth, and the estimated coefficients were trivial in magnitude. Inclusion of these two variables changes the reported coefficients only in minor ways. We also note that exports and diplomats sent are highly correlated ($r = .88$), and that entering either alone in the equations results in much larger slopes than does including both together.

ther analyses. We first added the block dummy variables to a lagged dependent variable model that included investment dependence per capita (1961–65) and domestic capital formation in 1960 as a percentage of gross domestic product (see Chase-Dunn 1975). We further incorporated all the control variables of equation (4) in table 4, as well as a measure of each country's degree of export concentration in 1965. The latter reflects the concentration of exports (the row entries in our original trade matrix) but not the origin-destination combination captured in the blockmodel partitions. Although the sample size is substantially reduced in this analysis, the effects of block location remain roughly as strong as those reported in table 4. In addition, some treatments (e.g., Chase-Dunn 1975) log transform the dependent variable in order to normalize the skewed distribution of GNP/capita or changes therein. That procedure substantially reduces the fit of the model as a whole (because the autoregression effect diminishes considerably), but the substantive conclusions based on the block-location coefficients are again unaltered by such transformations. We prefer the untransformed GNP/capita estimates (particularly given the absence of heteroscedasticity) because the metrics are so easily interpretable.

In summary, our extensive examination of other important determinants of economic growth identified in recent analyses indicates that the empirical effects of block location are in no way spurious. In our view, substantive interpretation of those effects as the accumulative structural advantage of the core over the periphery (and semiperiphery) of the world system is therefore warranted. As a final test of that interpretation, we estimate equation (2) in table 4 separately for changes in GNP/capita during the periods 1960–70 and 1965–70.[33] Table 5 presents results of those estimates, which may be compared with equation (2) in table 4. The effects of block location remain substantial (particularly for 1960–70). However, the absolute magnitudes of the coefficients of block location decline monotonically as the interval of observation decreases from 15 to five years.[34] These findings suggest that the advantages or costs of particular structural positions operate even in the very short run. But, while they are of modest size over a five-year period, these effects also persist through time and therefore result in

33 These analyses also address a potential concern with time ordering of the measures. Since the network data and block location variables pertain to the early 1960s, they are observed after some of the economic changes in the 15-year interval (1955–70) have already occurred. However, the findings reported below eliminate that as a plausible explanation for the results presented in table 4.

34 We also estimated expanded models of the form of equation (4) for changes from 1960 to 1970 and from 1965 to 1970. In these models, the coefficients of the block dummy variables are reduced by amounts roughly proportional to those shown in table 4. In the 1965–70 estimates, the effects for a few blocks (A, B, and F) become almost trivial in magnitude, but the general pattern of results supports conclusions identical to those based on table 4.

TABLE 5

ESTIMATES OF CHANGES IN GNP/CAPITA (1960–70 and 1965–70) REGRESSED ON CONTROL VARIABLES AND DUMMY VARIABLES REPRESENTING "STRUCTURAL POSITION" (Location in 10-Block Model)

	PARTIAL REGRESSION COEFFICIENTS (Standard Errors)	
INDEPENDENT VARIABLES	Change in GNP/Capita (1960–70) Eq. (5)	Change in GNP/Capita (1965–70) Eq. (6)
Initial GNP/capita (1960 or 1965)	.126 (.073)	.338 (.052)**
Secondary school enrollment ratio (1960)	14.90 (2.58)**	7.68 (1.84)**
Block A	−296.5 (143.7)*	−127.8 (96.6)
Block B	−317.7 (149.4)*	−136.8 (99.3)
Block C′	−482.0 (162.1)**	−262.5 (109.1)*
Block D	−425.3 (116.1)**	−210.5 (79.4)**
Block D′	−473.5 (143.2)**	−253.9 (97.1)**
Block E	−380.4 (159.9)*	−201.6 (107.3)
Block E′	−428.7 (153.4)**	−207.1 (103.6)*
Block F	−256.8 (124.0)*	−90.4 (90.1)
Block F′	−684.1 (174.8)**	−361.7 (114.7)**
Regression Constant	351.6	151.1
R^2	75.6%	83.2%

NOTE.—N = 105 nations for 1960–70; 106 nations for 1965–70.
* Significant at .05.
** Significant at .01.

large cumulative differentials in economic growth among nations even over a 15-year period.

CONCLUSIONS AND IMPLICATIONS

Taken together, the analyses reported here provide strong support for world-system/dependency theories of structure in the post–World War II period and of the sources of differential economic growth of nations. The blockmodel results indicate a core-semiperiphery-periphery structure in the world system. Moreover, the effects of structural position on the economic growth of nations from 1955 to 1970 are highly consistent with world-system/dependency formulations, although they do not differentiate between the economic costs of peripheral versus semiperipheral location. Substantively, these findings suggest that exogenetic theories of economic growth may be even more powerful than previous analyses have indicated. This is not to claim that world-system position alone determines the differential economic growth of nations. Both endogenous factors (education) and the magnitudes of some transnational flows (e.g., trade, interventions) have important effects and must also be included in models of economic growth.

More generally, this paper demonstrates a natural wedding of an empirically grounded theoretical approach to social structure (blockmodel

analysis) and a conceptual framework (the world system) that can be fruitfully applied to domestic conflict, inequality, and other substantive issues addressed by world-system treatments (e.g., Chirot 1977). In addition, we have begun designing blockmodel analyses of networks at several points in time which promise to answer questions concerning structural changes in the world system. For example, issues of obvious interest include the structural consequences of the incorporation of many African nations in the system circa 1960 and possible shifts in the location of OPEC countries in the 1970s. We do not mean to imply that such investigations are a substitute for the historical analyses and case studies that are necessary to understand national variations within structural positions and the mechanisms through which system position influences domestic processes. While we have not addressed such lines of inquiry here, our acknowledgment of their importance is more than token. Moreover, data availability will almost certainly limit analyses of the form reported here to the post–World War II period and will therefore preclude examination of long-run historical developments with our procedures. Despite this limitation, the approach employed here should inform research problems concerning the world political economy that we have not yet even considered.

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Some Economic Effects of Revolution: Models, Measurement, and the Cuban Evidence[1]

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While theorists have scrutinized the causes of revolution, they have avoided systematic study of its consequences. This paper counters such neglect in a number of ways. Rival models of the effects of revolution are developed. To make testing of these competing models possible, revolution is conceived of as an experimental treatment in an interrupted time-series design. Utilizing this design, economic data on the Cuban Revolution are analyzed. The findings reject three leading models of revolutionary effects—the Conservative, the Marxist, and the Thermidorian. Instead, the results hint that the economic effects of revolution are neither as pervasive nor as disruptive as commonly supposed, at least in the short run.

Revolutionary processes continue to fascinate students of collective behavior. A remarkable amount of attention has been devoted to the general issue of what causes revolution (see, e.g., Brinton 1938; Davies 1962; Gurr 1970; Johnson 1966; Moore 1966; Smelser 1963; Wolf 1969). Curiously, however, theoretical investigations concerning the effects of revolution are scarce. Moreover, the numerous case studies of specific revolutions tend to eschew global models of effects. In fact, Eckstein (1964, p. 28) argues that no questions about revolution "have been more thoroughly neglected by social scientists" than those about consequences. Some 10 years later, Greene (1974, p. 11) echoes this conclusion, observing that "much remains to be done in this area of study, especially from the perspectives of social science." As both critics further note, the few Western scholars who have formally considered revolutionary outcomes have, with the obvious exception of the Marxists, provided largely negative evaluations (Eckstein 1964, p. 2; Greene 1974, p. 11). Through his emphasis on the extensive damage wrought by revolution, Huntington (1968, pp. 308–10) appears a contemporary representative of this tradition.

What explains the comparative lack of research on general models of

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revolutionary effects? Of course, it is difficult to say, but one reason, suggested by Eckstein (1964, p. 6), seems especially important. He argues that revolutions, or "internal wars" in his phrase, are not well suited to the techniques of modern social science, such as social surveys, controlled experiments, and anthropological field work. As he puts it: "Many of them do occur, but nowhere near enough to provide the number of cases one deals with, say, in voting studies. They can hardly be staged experimentally, and tend, for obvious reasons, to deter field workers, interviewers, and pollsters" (1964, p. 6). Revolution does appear to resist analysis, at least through these approaches. Nevertheless, developments in evaluation research suggest a way out of this methodological quandary.

The work of Campbell (1969), especially, has encouraged policy evaluators to regard social reforms as experiments, or quasi experiments. I propose a similar view for revolutions. Such an experimental focus yields a number of benefits. First, consideration of revolution as an experimental treatment immediately fixes attention on outcomes. Second, the particular quasi-experimental approach employed here, that is, an interrupted time-series design, promises a quantitative assessment of revolutionary impacts. Further, this statistical model can be used to test rival theoretical models of the effects of revolution.

In what follows, I begin by formulating three competing models—Conservative, Marxist, and Thermidorian. The models are more formally expressed as possible outcomes in an interrupted time-series design and are tested with Cuban data, which direct attention to the economic effects of revolution. I will look at the revolution's impact on sugar production first and then at economic growth in general.

THEORETICAL AND STATISTICAL MODELS OF REVOLUTIONARY EFFECTS

Because revolutions aim to transform ongoing social processes, they tend to be disruptive events. The theoretical concern here is how revolution disrupts valued social and economic production, in the short run and in the long run. Suppose observations of a nation's economic production fall along a line, as in figure 1, where the x-axis stands for time (T) and the y-axis represents economic production (EP). One observes production as a perfect, positive linear function of time, $EP = f(T)$.

When revolution (R) occurs, the classic hypothesis of impact, for the short run, posits a more or less immediate drop in the level of economic production, as diagramed in figure 2. From this perspective, revolution has an initial cost, indicated by the difference between A' and A. Of course, the usual expectation is that the impact of revolution survives beyond this first jolt. In fact, a common prediction would be that the revolution would introduce structural changes which would alter social and economic output

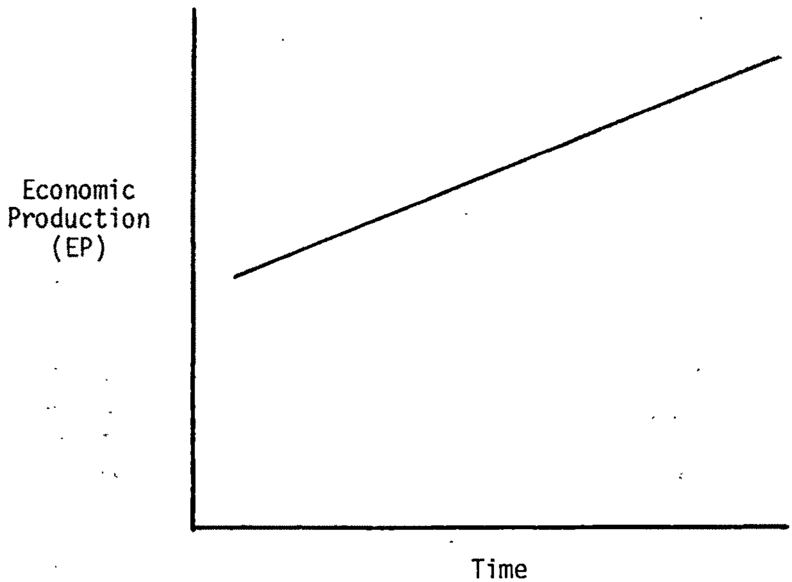

FIG. 1.—No revolution

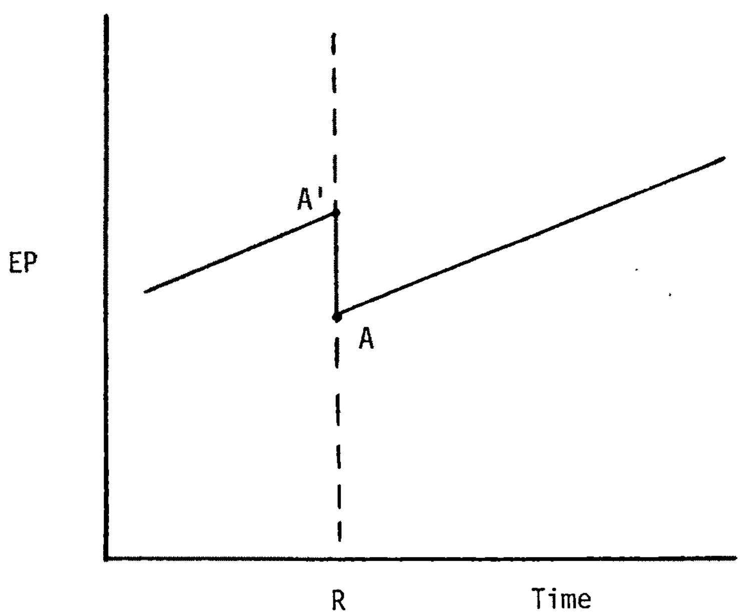

FIG. 2.—Short-run, costly revolutionary effect

in a lasting way. Diagrammatically, these long-run effects appear as a change in the slope (S) of the production trend line following the revolution. Within a linear framework, there are three possibilities: the slope may increase, decrease, or remain the same (see fig. 3). Joining these alternative slope changes ($\Delta_S = +, -, 0$) with the negative change in level ($\Delta_L = -$) yields three models of revolutionary effects. For convenience, I have labeled them Conservative, Marxist, and Thermidorian. I would like to discuss the Conservative model first.

In the Conservative view, revolution is costly, both in the short and long run. This perspective appears to be widely held by Western scholars (Eckstein 1964, p. 2; Greene 1974, p. 11). For example, it would seem congruent with Huntington's (1968, pp. 308–9) assertion that the "economic results of a revolution are almost entirely negative." Beyond the initial disruption of productive forces, revolution inflicts enduring harm on a nation's capacity to produce. The adverse result of the first revolutionary shock is indicated by a drop in economic production: $A' - A$. The diminished slope of the line after the revolution represents the reduced ability of the nation to produce. Revolution, then, becomes a costly endeavor, both at the beginning and indefinitely into the future. Even the prerevolutionary level of output, at A', is not finally restored until point B. Thus, because of these joint short- and long-term effects, the detrimental influence might even extend over generations.

Perhaps the chief theoretical rival to the Conservative model is a Marxist one. According to Marxist theory, revolution creates necessary, substantial short-run costs but yields important long-run benefits. The Marxist revolu-

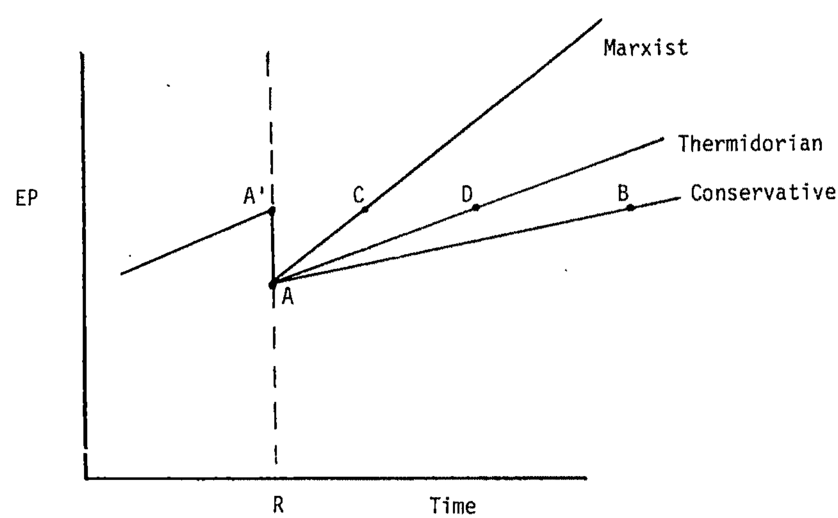

FIG. 3.—Rival models of revolutionary effects

tion, in reordering the ownership of the means of production, generates extreme social and economic dislocation, which immediately hurts production (as with the Russian case). However, the new relations established in the socialist society engender greater production of valued economic and social goods than was possible before.[2] In this model, the initial revolutionary drop in output is precipitous ($A' - A$), but the new production curve is much steeper than previously. By point C production is back up to the prerevolutionary level. Still later, the new revolutionary forms have made output higher than would have been attainable without the revolution. Hence, according to the Marxist view, revolution makes up for its preliminary costs, ultimately serving to benefit society.

A third perspective, inspired by Brinton's (1938, chap. 8) work on revolution, is the Thermidorian model. It differs from the previous two by emphasizing neither costs, as the Conservative model does, nor benefits, as the Marxist one does. Rather, the Thermidorian model stresses the return to "normalcy" which occurs after the initial revolutionary losses. At the beginning, there is the inevitable fall in production level (which is overcome by point D). However, prerevolutionary production rates are soon restored, a condition indicated by the absence of a slope change. This view of revolution occupies somewhat of a middle ground, for although development is delayed the society is brought back to health, to draw on Brinton's (1938, pp. 24–26) analogy.

There are, of course, other theoretical possibilities, beyond the Conservative, Marxist, and Thermidorian models diagramed in figure 3. While it seems likely that revolution exacts an initial production cost, one could postulate that the short-run effect is neutral ($\Delta_L = 0$) or even beneficial ($\Delta_L = +$). Further, it may be more realistic to suppose that the initial impact, whatever the direction, would be delayed, rather than immediate; for example, production may fall a year or two after the revolutionary takeover, as parts wear out and managers and technicians leave. In addition, the production curve might not be linear, as assumed. For instance, output over time often follows an exponential path. These alternative model specifications—a nonnegative change in level, a lagged short-run effect, a curvilinear production trend—are entertained later on. Thus, the theoretical structures in figure 3 serve as points of departure.

While the verbal description is, necessarily, sometimes vague or only illustrative, the models themselves provide rather precise rival predictions about revolutionary effects. How can they be tested with actual revolutionary data? Development of a satisfactory statistical model of revolutionary effects requires that the notion of "revolutions as experiments" be

[2] Some analysts may be concerned about spuriousness, worrying that production changes might in fact be caused by productivity changes, rather than by the revolutionary treatment. However, any productivity changes are more appropriately viewed as part of the revolutionary treatment and, as such, could (rightly) account for some of its effect.

thoroughly explored. That is, revolutions ought to be consciously evaluated as social "treatments" with statistically analyzable effects. Following Campbell (1969), who urges this view of reforms, a revolutionary treatment would be no more than quasi-experimental (Cook and Campbell 1976, pp. 223–24). Affirming statistically significant relations can be problematic in the quasi-experimental design of interrupted time series (ITS), which is employed below.

Suppose that within an extended series of annual observations on economic production, a relatively discrete event, a revolution, occurs. I may observe that the level and rate of production appear to drop after the revolution (as the Conservative model would predict). The central statistical question is whether these production differences before and after the revolution are merely due to chance. More formally, are these changes in the level and slope of the production curve statistically significant, thereby ruling out (within acceptable probability limits) the possibility that they are random fluctuations?

This query can be answered through proper estimation of the following linear regression model (Campbell and Cook [1978] propose that an equation of this form generally be used to analyze the effect of intervention in a time series):

$$Y_t = b_0 + b_1X_{1t} + b_2X_{2t} + b_3X_{3t} + e_t\,, \qquad (1)$$

where Y_t = yearly observations on economic production; X_{1t} = a counter for years from 1 to N, the number of observations; X_{2t} = a dichotomous variable scored 0 for observations before the revolution and 1 for observations after the revolution; X_{3t} = a counter of years scored 0 for observations before the revolution and 1, 2, 3, . . . for observations after the revolution; b_0, b_1, b_2, b_3 = parameters to be estimated; and e_t = error.

I would like to examine the meaning of the parameters in the model. The parameters b_0 and b_1 estimate, respectively, the level and the slope of the time series before the revolution. The concern here is if the intervention of the revolution affects either of these parameters. Therefore, attention turns to b_2, which estimates the change in level of the series after the revolutionary event, and b_3, which estimates the change in slope after the revolutionary event. The null hypotheses of no change are, respectively, $H_0: b_2 = 0$ and $H_0: b_3 = 0$. If the coefficient b_2 is not significantly different from zero, one can reject the hypothesis of short-run revolutionary effect. Likewise, if b_3 is not significant, the hypothesis of long-run revolutionary effect can be rejected. These hypotheses are tested in the data analysis which follows.

THE EFFECTS OF THE CUBAN REVOLUTION: SUGAR PRODUCTION

Beyond a doubt, the Cuban Revolution has profoundly changed Cuban society. But there are difficulties in fully quantifying the change, owing to

data problems typical of revolutionary situations and of Third World countries generally. For example, the shortage of observations on health, education, and income distribution before and after the revolution precludes reliable ITS analysis. Omission of these critical categories, in which the revolutionary government is generally acknowledged to have done much, immediately circumscribes my findings. Therefore, I wish to recognize at the outset these important contributions in order to avoid understating the revolution's accomplishments (see Matthews 1975, chaps. 15 and 16). Fortunately, the data limitations are less severe with regard to economic production, where an evaluation of the revolution's impact seems possible. Initially, I consider sugar production and then go on to a more general indicator of Cuban economic performance.

The importance of sugar for Cuba's economy is hard to exaggerate. As far back as 1894, a production of over 1,000,000 tons was reported (Williams and Wright 1975, p. 60). Throughout the 20th century, sugar has dominated Cuban exports and generated a high portion of national wealth. In the period immediately before the revolution (1949–58), sugar made up 84% of the exports and provided 28%–29% of the GNP (Mesa-Lago 1971, p. 278). And, apparently, its impact yet exceeds that demonstrated by these impressive figures. As Henry Wallich explains, the "tendency to pin all hopes on the international sugar markets gives sugar an economic and political dominance even greater than its true weight in the economy" (1950, p. 12). Despite an early effort by the Castro regime to overcome the sugar dependency, it continued to be Cuba's economic backbone (Boorstein 1968, chap. 6; Mesa-Lago 1974, p. 56). Even after failure to achieve the heralded 10,000,000 ton *zafra* in 1970, Castro himself announced: "The fact remains that the nation's basic resource is sugar. . . . And we say again that ours is an economy which depends on one product—sugar cane" (*Granma Weekly Review*, December 20, 1970, pp. 4–5). Further, the leaders of the central planning agency, JUCEPLAN, have indicated that the Cuban economy must concentrate on sugar for some time to come (Matthews 1975, p. 370).

An analysis of sugar output suggests itself not only because of sugar's central economic role, but also because sugar statistics appear more reliable than those on other production and consumption variables, as evidenced by their broad acceptance in the Cuban literature. Nevertheless, Mesa-Lago (1969*a*, p. 59; 1969*b*, pp. 53–54), while accepting the figures up to the mid-1960s, argues that sugar data have been suspicious since 1965, when some inflation may have begun. Such distortions, if they occurred, do not appear to have been serious, for the analysis below reveals no upward bias in the reports from 1965 onward (i.e., actual production was as likely to be below, as above, its predicted value). Beside its strength as a measure, sugar production has the statistical advantage of being the longest available series

(N = 55), which provides more stable estimates and a greater likelihood of detecting change.

Figure 4 plots annual sugar production (in thousands of metric tons) from 1920 to 1974 (on data sources, see n. 7). The broken line intersecting the series marks the revolutionary takeover in 1959. More precisely, Batista formally governed until December 31, 1958, when he fled the island. Castro's assumption of control dates from January 8, 1959, when he triumphantly entered Havana (Karol 1970, p. 572). Therefore, production reflected in the 1959 observation occurred almost entirely under Castro's revolutionary government, whereas production in 1958, and in the years prior, took place completely under prerevolutionary regimes.

Estimating the ITS model, initially with ordinary least squares (OLS), yields the following:

$$\begin{aligned} Y_t = \underset{(8.93)}{3308.71} + \underset{(2.60)}{43.62}X_{1t} + \underset{(.56)}{408.35}X_{2t} - \underset{(-.34)}{22.73}X_{3t} + e_t, \\ R^2 = .308, \quad d = .838, \end{aligned} \tag{2}$$

where the values below the parameter estimates = the t-ratios; R^2 = the coefficient of multiple determination; d = the Durbin-Watson statistic; Y_t = yearly sugar production (in thousands of metric tons); X_{1t} = years from 1, 2, . . . 55; X_{2t} = 0 (before 1959), 1 (1959 and after); X_{3t} = 0 (before 1959), 1 (at 1959), 2 (at 1960), . . . 16 (at 1974); and e_t = error.

The coefficients of equation (2) imply that there is no significant effect on sugar production from the revolution, either in the short or long run. However, before accepting this judgment, adjustments should be made for several problems which frequently complicate ITS analysis and which these OLS estimates highlight. The difficulties are as follows: autocorrelation, nonlinearity, and specification of the intervention point. To begin, I address the autocorrelation problem.

Supposing other necessary assumptions are met, OLS will yield desirable estimators when the error terms are uncorrelated, $E(e_ie_j) = 0$, $i \neq j$. For equation (2), $d = .838$, which suggests rather severe autocorrelation difficulties. To adjust for this autocorrelation, I assume a first-order autoregressive model of the error terms, $e_t = \rho e_{t-1} + u_t$, which holds that present error, e_t, is a function of prior error, e_{t-1}, plus some random effect, u_t, that is independent of e_{t-1}. (The appropriateness of this first-order autoregressive assumption and the general problem of autocorrelation in ITS analysis is fully discussed in the footnote.)[3] When error terms are generated by this

[3] I rely on a standard econometric approach in adjusting for autocorrelation. That is, I assume a first-order autoregressive model of the error terms and correct for it. Adoption of a first-order autoregressive model—AR(1) in Box-Jenkins (1970) terminology—is not a serious constraint. An AR(1) process dominates annual economic time series, which I have here (Ames and Reiter 1961; Kmenta 1971, p. 278; Pindyck and Rubinfeld 1976, p.

process, different correction procedures are available. I selected the Cochrane-Orcutt (CORC) technique, which produces desirable estimators and is straightforward and intuitively pleasing. To oversimplify, CORC first estimates the correlation of the error terms, $\hat{\rho}\hat{e}_t\hat{e}_{t-1}$; accordingly, it corrects each observation, subtracting an amount determined by the autocorrelation estimate. These steps are repeated until the estimates converge. By this iterative procedure, estimates equivalent to maximum likelihood estimators are eventually obtained (for a more mathematical explication of the CORC procedure, see Kmenta 1971, pp. 287–89).

The problem of nonlinearity is suggested by a visual inspection of the annual sugar output, which, especially in the earlier years, fluctuated with U.S. prosperity and wartime involvement. Also, it is implied by the modest R^2. Finally, examining the plot of residuals confirms the presence of significant curvilinearity, which can be coped with in various ways. I chose to apply a natural logarithmic transformation to Y_t.

With respect to the specification of the intervention point, the formal transfer of power from Batista to Castro is clearly fixed in time. But, how soon can revolutionary effects be expected to appear? In the Cuban case, as with other revolutions, there was a transition phase. Although exact dates are difficult, 1961 saw the full radicalization of the revolution and the adoption of a "Soviet model" (Ritter 1974, chaps. 3–4). One aspect of this

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472). To evaluate this procedure, I examine the corrected level of autocorrelation, using the Durbin-Watson test statistic. (Past research with ARIMA models has employed the Durbin-Watson test as a general evaluation technique [see Nelson 1973, pp. 214–15].) According to the Durbin-Watson value from the Cochrane-Orcutt estimation of eq. (3), autocorrelation ceases to pose a significant threat. The autocorrelation problem could be less tractable in other ITS analyses, especially with nonannual, noneconomic observations. Solutions applied in past examples have been inadequate (Campbell and Stanley 1963; Caporaso and Pelowski 1971; Caporaso and Roos 1973; Duvall and Welfling 1973). For certain kinds of time-series analysis, the Box-Jenkins (B-J) forecasting technique offers a flexible procedure for modeling error processes (Box and Jenkins 1970; Hibbs 1974; Nelson 1973). But proposals to apply these B-J methods specifically to ITS analysis often do not fully take into account the difficulties (e.g., Campbell and Cook 1978; Glass, Willson, and Gottman 1975). The B-J technique provides a forecasting model, whereas the ITS method examines the causal impact of a real world variable on Y. The causal implications of an ITS design undermine a critical assumption required for B-J procedures, i.e., stationarity of the time series. When a time series, z_t, is stationary, one implication is that the expected values of any two observations are the same, $E(z_t) = E(z_{t+m})$. Therefore, the mean of a series should be a constant, independent of time (see Nelson [1973, pp. 19–23] for a useful discussion of stationarity). The dilemma for an ITS design becomes apparent immediately. When an intervention has an effect (e.g., an intercept change), the mean of the series should shift from a pretreatment to a posttreatment level, rendering the series nonstationary. (Further, this sort of nonstationarity is not homogeneous and cannot be removed by differencing.) These particular difficulties are just beginning to be considered in the statistical literature, and significant practical problems of modeling and estimation remain (see Box and Tiao [1975] and Hibbs [1977*a*, 1977*b*], who has advanced the work here significantly). Finally, as Campbell and Cook (1978) themselves admit, economists have shied from a B-J approach because of its treatment of the stationarity question.

economic restructuring was a de-emphasis on sugar, as the revolutionary leadership sought to transform Cuba from an agricultural to an industrial state. The precipitous drop in sugar output from 1961 to 1962, shown in figure 4, suggests the consequences of this fundamental reorientation. Further, it argues that the short-run revolutionary impact is more fully observed in 1962, rather than in 1959 (see the solid line, fig. 4).

In view of the above considerations, I use the CORC procedure to re-estimate the ITS equation, after having logged sugar production (to the base e) and respecified the short-run effect hypothesis (hence, for X_{2t} and X_{3t}, 1962 = 1):

$$\begin{gathered} \log_e Y_t = 7.885 + .019X_{1t} - .328X_{2t} + .009X_{3t} + e_t, \\ (29.24) \quad (1.92) \quad (-1.54) \quad (.24) \\ R^2 = .678, \quad d = 2.197. \end{gathered} \tag{3}$$

These estimates, arrived at after two iterations, reflect adjustments for the high autocorrelation that affected this series, $\hat{\rho}\hat{e}_t\hat{e}_{t-1} = .737$. The Durbin-Watson statistic indicates that autocorrelation is no longer a problem. Further, nonlinearity has ceased to be troublesome. Besides the improvement in "fit," an inspection of the residual plot reveals no significant curvilinearity (i.e., the residuals are more or less randomly scattered across the time span).[4] These tests encourage acceptance of the estimates in equation (3) (further assurance comes from ruling out a possible multicollinearity threat).[5]

[4] In general, the closer the Durbin-Watson statistic, d, is to 2, the better. But the precise value for d at which the null hypothesis of no autocorrelation is accepted varies with the level of significance, whether the test is one- or two-tailed, the sample size, and the number of independent variables. Here I have three independent variables, a 5% level of significance with a two-tailed test, and $N = 54$ (one of the original cases is necessarily lost with the CORC adjustments). Under these conditions, if the value of d lies between 1.60 (d_u) and 2.40 ($4 - d_u$), as it does in this case, then the null hypothesis of no autocorrelation can be accepted (see Kelejian and Oates 1974, pp. 200–3, 278). Interestingly, with an ITS design, the Durbin-Watson test serves as a check on nonlinearity as well as autocorrelation. One test of linearity is whether residuals, when arrayed according to X, are randomly scattered around the regression line. If X is a counter of time, as with ITS, then the Durbin-Watson test, by indicating that errors are uncorrelated, also suggests the absence of nonlinearity (see discussion in Kmenta 1971, pp. 470–71). Of course, the implication is that an autocorrelation correction procedure, such as CORC, will tend to remove nonlinearity as well.

[5] While a multicollinearity problem is suggested, a rather demanding test shows that there is none. The correlation between X_{2t} and X_{3t} for the sugar production series is high, $r = .85$ (a coefficient of this magnitude comes from the very large number of prerevolutionary scores here for X_{2t} and X_{3t}, which the ITS model sets at zero). This implies that the estimates of eq. (3) could be wildly off, because the effects of X_{2t} and X_{3t} are not being reliably separated. When multicollinearity is operating in this way, solution is problematic. One approach is to analyze the effects of X_{2t} and X_{3t}, in turn, as follows:

$$\begin{gathered} \log_e Y_t = 7.854 + .021X_{1t} - .310X_{2t} + e_t \\ (32.15) \quad (2.53) \quad (-1.59) \end{gathered}$$

Although the coefficient for the short-run effect, b_2, is in the expected direction, it is not statistically significant (at the .05 level, for a one-tailed test, $|t|$ must exceed 1.65). Thus, responsibility for a sugar production drop from 1961 to 1962 cannot be confidently attributed to the revolutionary government. Possibly, the decline would have occurred under the pre-revolutionary government as well. (Also, as expected, no short-run effects for the revolution emerge when the other possible intervention points are analyzed, i.e., when for X_{2t} and X_{3t}, 1 = 1959, 1960, 1961, or 1963, b_2 does not approach statistical significance.) With regard to the long-run impact of the revolution, although b_3 is positive, the t-ratio indicates that it is far from statistical significance. (Likewise, when other intervention points—1959, 1960, 1961, 1963—are examined, the revolution does not come close to achieving a statistically significant long-term effect.)

In his recent book on Cuba, economist Mesa-Lago singles out sugar, asserting that it "has had a discouragingly bad performance under the Revolution" (1974, p. 56). While this evaluation is more nearly correct for the short run (1962), it certainly appears false for the long run. Indeed, if I were to generalize, in the fashion of Mesa-Lago, I would have to conclude that the revolution has had no effect on sugar production. What is more surprising, however, is the degree to which sugar production levels resist change, even in the face of serious government initiatives. This finding has profound implications for the formulation of Cuban development policy. It suggests that the best efforts of any Cuban government can heighten sugar output only marginally. Sugar production capacity may have reached a point where it is highly constrained by factors that the government cannot greatly alter, such as the island's agricultural and human resource base, not to mention international patterns of trade and dependency.

These findings fail to support any of the models of effect postulated earlier, although equation (3) tends to the Thermidorian prediction. However, the evidence on sugar may mislead for, while very important, sugar represents only one component of Cuban economic production. To speak with some assurance about the revolution's global impact on economic growth, it is necessary to analyze a more aggregate, GNP-type indicator.

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and

$$\log_e Y_t = \underset{(31.30)}{7.947} + \underset{(1.68)}{.016X_{1t}} - \underset{(-.38)}{.012X_{3t}} + e_t .$$

The revolution still manages to exhibit no statistically significant effects, even allowing each intervention variable to explain all of Y_t it can, free of the control of the other. Furthermore, their t-ratios and parameter estimates are quite close to those in the full ITS equation. Therefore, I conclude that multicollinearity does not pose a serious threat to the interpretation of the eq. (3) results.

THE EFFECTS OF THE CUBAN REVOLUTION: ENERGY CONSUMPTION AND ECONOMIC GROWTH

While there is broad recognition of the revolution's beneficial social consequences, for example, improved social services, medical care, and educational opportunities, sharp disagreement exists about its overall effect on economic production. Among those who contend that the revolution has had a favorable economic influence, a "stagnation thesis" is popular (Boorstein 1968, chap. 1; Matthews 1975, pp. 35–39, 292–318; O'Connor 1970, pp. 17–18; Roca and Hernandez 1972, p. 68; Seers 1964, pp. 3–19). According to this perspective, prerevolutionary Cuba had shown almost no signs of economic growth as far back as the 1920s. The revolution eliminated the patterns that produced this stagnation and initiated a period of lasting, significant economic advance. But there are others who claim, for a variety of reasons, that the revolution has not had an entirely salubrious effect on Cuba's economic development (Dumont 1970; Gonzalez 1974, pp. 193–218; Karol 1970, pp. 407–84; Nelson 1972). One of Castro's strongest critics, Lowry Nelson, states his opinion quite flatly: "Cuba constitutes a unique example of a country launched on a classical linear path of capitalist development, a course that is suddenly truncated by revolution. . . . The consequence to Cuba has been an almost unmitigated economic disaster" (1972, pp. 36, 196). My analysis does not fully support any of these opposing generalizations.

To respond to the question of the revolutionary impact on economic growth, the focus should be on indicators of central tendencies in the Cuban economy. Of course, GNP figures would be ideal. Unfortunately, in addition to gaps in the series, the change in computation of national accounts from capitalist to socialist techniques rules out reliable comparison with prerevolutionary figures (Mesa-Lago 1969*b*, pp. 48–51; 1971, pp. 323–24). Faced with such a dilemma, one must seek indicators which are serviceable proxies for GNP.

David McClelland, in his imaginative work *The Achieving Society*, decided that electric energy production reliably indicated a nation's economic production (1961, pp. 82–87). McClelland selected electricity output as an indicator because he lacked adequate national income figures, as is also the case here. He reasoned that most of the productive machinery of contemporary societies would not run without electrical power (pp. 85–87). Therefore, electric output, as measured in kilowatt hours, would be an indicator of a nation's material production. This rather novel measurement appears to have substantial empirical validity, as evidenced by a correlation of .865 between per capita electricity production and the Watkins-Hagen estimates of per capita national income for a sample of 40 countries (McClelland 1961, p. 87). Hence, using electrical energy data to represent GNP, it may be possible to assess indirectly the revolution's contribution to

Cuban economic growth. A series on electrical energy production can be assembled. However, the observations represent only public production, not total production (Mesa-Lago 1969*b*, p. 59). Since nationalization by the revolutionary government expanded the state sector, these figures would at least partially reflect the change to socialism, rather than real economic growth. But, there are also data on energy consumption, which McClelland equates with energy production (1961, p. 86). These figures on energy consumption include all consumption, public or private, and are thus not subject to the shortcomings of the energy production figures.

In the absence of adequate national accounts data, energy consumption is not a totally unreasonable proxy. Besides the empirical overlap with national income, indicated by McClelland, it has the advantage of being an unobtrusive measure. Energy consumption figures seem much less likely to be "fudged" than GNP figures, which easily lend themselves to propaganda purposes. Critic Mesa-Lago (1969*b*, p. 58) appears to accept electric energy statistics as accurate, thereby giving support to this inference. Further, an ITS analysis will resist various common sources of error.[6]

In addition to the foregoing arguments, the Cuban data themselves suggest that the energy consumption variable is tapping GNP. First, energy consumption is highly correlated with production in key Cuban industries for which data are available: with cement production, $r = .84$; cigarette production, $r = .72$; crude petroleum production, $r = .77$; motor fuel production, $r = .70$; kerosene production, $r = .94$; phosphate fertilizer consumption, $r = .80$; nitrogenous fertilizer consumption, $r = .86$; potash fertilizer consumption, $r = .79$.

More general evidence comes from a principal components analysis of all the production and consumption variables (23) on which I could assemble a more or less adequate series.[7] While the first factor accounts for 53% of

[6] Under a condition of constant error, e.g., figures from the Castro and Batista regimes are equally inflated, ITS estimation of significant pretreatment to posttreatment change remains unaffected (Costner 1971, pp. 299–303). Mesa-Lago himself argues that regimes as opposing as those of Castro and Batista tend to generate the same biases in statistical information (1969*b*, p. 75).

[7] An interrupted time series must have "enough" pretreatment and posttreatment observations to permit meaningful application of the necessary statistical tests. The average series length is 37 observations for these Cuban data, with the last observation always from 1973 or 1974. With regard to missing data, the aim was to include just those series which contained no gaps (minor interpolation of one or two values was necessary for beer production, cigarette production, nitrogenous fertilizer consumption, motor fuel production, kerosene production, and phosphate fertilizer consumption). In addition to these, series were also gathered on the following production variables: beans, cement, chromium ore, cocoa, coffee, copper ore, corn, crude petroleum, electric energy, peanuts, rice, salt, sugar, and tobacco; and on these consumption variables: energy, newsprint, and potash fertilizer. These total to 23 variables. The agricultural production data were gathered from volumes of the following statistical yearbooks: *International Yearbook of Agricultural Statistics* (Rome: Institut International d'Agriculture, 1922–47); *Yearbook of Food and*

the variance in the data, the remaining factors individually contribute little (e.g., the second factor adds only 10% more to the variance explained). Perhaps this first factor, which represents the best linear combination of all 23 variables, is a partial measure of national production. Interestingly, the variable having the highest factor loading is energy consumption, with .97.[8] This extremely high loading indicates that, at least empirically, the energy consumption variable is equivalent to this production factor. Thus, I have some confidence that energy consumption is a workable substitute for GNP.

In figure 5, 26 annual observations on energy consumption from 1949 to 1974 are plotted (the values are natural logarithms of actual consumption, measured in millions of metric tons; data sources are described in n. 7). The revolutionary intervention is indicated by the broken vertical line between 1958 and 1959.

Estimating the ITS model with OLS yields

$$\begin{aligned} \log_e Y_t &= \underset{(35.35)}{.836} + \underset{(20.06)}{.076X_{1t}} + \underset{(3.26)}{.089X_{2t}} - \underset{(-7.43)}{.032X_{3t}} + e_t, \\ R^2 &= .995, \quad d = 1.702. \end{aligned} \tag{4}$$

According to these OLS estimates, there are significant changes at the .05 level (two-tail, $|t| > 2.06$) in the level and slope of the energy consumption curve after the revolution, as indicated by b_2 and b_3. Further, the Durbin-Watson statistic indicates the absence of autocorrelation, allowing the acceptance of these significant results as unbiased. A diagram nicely illustrates the meaning of these changes and the workings of the ITS design. I would like to consider the slope change first. In figure 5, the predicted consumption values from the above multiple-regression equation are linked together, first for the preintervention years and then for the postintervention years. The result is two straight lines, which actually provide simple linear regression fits to the points within the particular time period. Visually, the slopes of the two lines appear to differ. In fact, the slope of the earlier line is .076 (b_1), while the slope of the later line is only .044. This drop in slope of .032 is indicated by b_3 and is statistically significant.

Agricultural Statistics, pt. 1 (Rome: Food and Agricultural Organization of the United Nations, 1949–57); *Production Yearbook* (Rome: Food and Agricultural Organization of the United Nations, 1958–74). The remaining data were gathered from volumes of the *United Nations Statistical Yearbook* (New York: Statistical Office of the United Nations, 1948–75).

[8] This energy consumption loading is on the first factor of a principal component analysis (unrotated) of the above 23 variables (logged to the base e). This loading remains almost exactly the same, regardless of whether the variables are unlogged, the factors are rotated (varimax), or the energy production variable is excluded. Further, the percentage of variance explained by the first factor is stable under these different conditions.

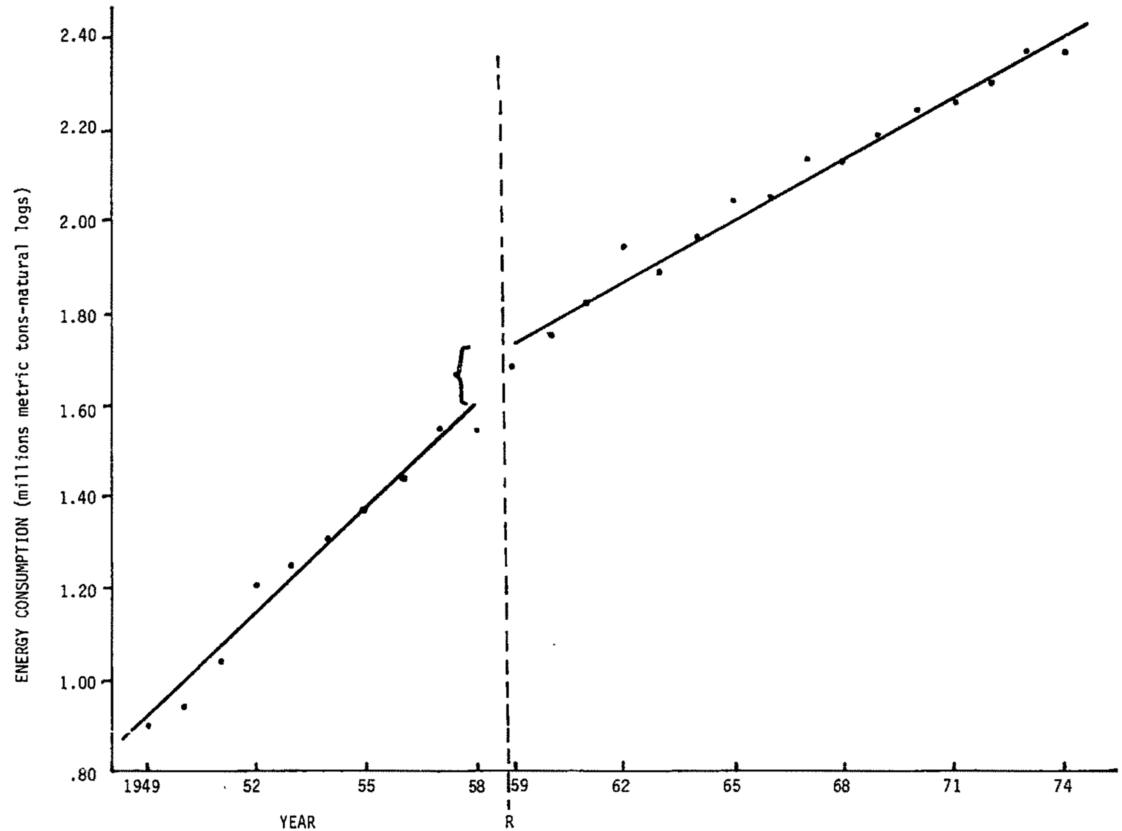

FIG. 5.—Interrupted time-series analysis of Cuban energy consumption (1949–74)

Turning to the level, the change here is suggested by the breaking of the two lines between the 1958 and the 1959 values. For the prerevolutionary years, the prediction equation reduces to $\log_e \hat{Y} = b_0 + b_1X_{1t}$ (because X_{2t} and X_{3t} take on zero values). Supposing the revolution had not occurred, the prediction for 1959 based on this prerevolutionary model would be 1.6763. However, the revolution did occur, and the corrected prediction for 1959 becomes 1.7337, showing an increase of .0574. This increase is caused by the significant postintervention intercept rise (b_2) and would be even greater if the depressing effect of the negative slope change (b_3) were not operating ($b_2 - b_3 = .0889 - .0315 = .0574$).

As noted, the observations on energy consumption are expressed in "natural" logarithms (logs to the base e), while the independent variables, which are various measures of time, remain unlogged. This semilog transformation is especially useful when the concern is with assessing rates of economic growth. It tends to "straighten out" the observations, which in their raw form usually follow a curve that steepens over time. This linearization, of course, makes an OLS analysis more appropriate. But, in addition, the transformation allows the evaluation of percentage change, rather than absolute change. For example, if OLS were used to estimate a simple exponential model predicting energy consumption from time (in years), $\log_e Y_t = b_0 + b_1X_{1t} + e_t$, the interpretation would be that a one-year increment yields a ($b_1 \times 100$) percentage increase in energy consumption.[9]

With this understanding, the ITS estimates for energy consumption in equation (4) can be more fully considered. The slope, b_1, estimates that energy consumption grew at a rate of 7.6% a year during the prerevolutionary period. As b_3 indicates, after the revolution this rate was dampened by 3.2%, a significant decline. Therefore, under Castro the estimated annual energy consumption rate stands at 4.4%. How do these estimates compare with those of GNP growth? Ritter, in response to the serious data limitations, remarks: "Research on the Cuban economy is thus reduced to a type of detective work" (1974, p. vii). I proceed in this spirit.

The only reliable national accounts statistics for the prerevolutionary period, according to Mesa-Lago (1969*b*, pp. 48–49), came from the National Bank, which reported an annual GNP growth rate in current prices for 1950–58 of 4.6%. However, the preferred comparison would actually seem to be with GNP growth in constant prices, since energy consumption is measured in units of constant value (i.e., metric tons). Although the National Bank did not report GNP in constant prices, consumer price indices from Havana implied an inflation of 1.1% a year (Mesa-Lago 1971, p. 278). This figure suggests that the annual growth rate, in constant prices, was around 3.5% for the period. Accepting this estimate, one might infer a

[9] A proof for this interpretation, which becomes problematic as b_1 exceeds .25, is found in Tufte (1974, pp. 124–26).

constant average positive error of 4.1% a year (7.6% − 3.5%) when using energy consumption growth to estimate real GNP growth. (This supposition is not entirely arbitrary. Even if GNP and energy consumption were perfectly correlated, the energy consumption–GNP conversion might well be inefficient.) Therefore, GNP growth, at constant prices, under the revolutionary government would be more correctly estimated at .3% annually.

However, this estimate in some sense exaggerates economic growth, for it does not control for population increase. Therefore, I reestimate (OLS) the ITS equation, but with per capita energy consumption (logged to the base e) as the dependent variable:

$$\log_e Y_t = \underset{(253.95)}{6.115} + \underset{(13.33)}{.052X_{1t}} + \underset{(3.17)}{.088X_{2t}} - \underset{(-6.55)}{.028X_{3t}} + e_t, \tag{5}$$
$$R^2 = .987, \quad d = 1.720.$$

While the pattern remains the same as in equation (4), the coefficients are reduced, as would be expected. Beginning with the National Bank statistics noted above, the growth of GNP per capita for 1950–58, in constant prices, can be estimated at 1% annually (the period's annual population growth rate of 2.5% subtracted from the real GNP rate [see Mesa-Lago 1971, p. 278]). If this estimate is correct, it suggests an average constant positive error of 4.2% a year (5.2% − 1.0%) in using per capita energy consumption growth to predict real per capita GNP growth. Applying this adjustment for the Castro period yields a real annual per capita GNP growth rate estimate of −1.8% (revolutionary growth rate = [5.2% − 2.8%] − 4.2% = −1.8%).

Comparison with more standard, although less comprehensive, analyses suggests that this estimate of economic growth under the revolution is not completely fanciful. United Nations figures for 1962–66 indicate that real GNP per capita fell .5% per annum (Mesa-Lago 1969*b*, p. 5). For the period 1960–70, the World Bank reports a real GNP per capita growth rate of −.6% a year (World Bank Group 1972, p. 3). Another evaluation, for 1960–69, shows a real GNP per capita decrease of 3.2% yearly (International Monetary Fund 1972, p. 51). Ritter, however, argues convincingly that this last estimate is probably an exaggeration (1974, p. 209). On the basis of these investigations, my estimate of an annual 1.8% decline in real GNP per capita for the entire period, 1959–74, would not appear wide of the mark. Its credibility is enhanced further upon consideration of the meaning of the significant intercept shift (b_2).

For the period of revolutionary transformation, there is rather broad agreement that economic performance, especially in 1959, was better than in the prerevolutionary period (Mesa-Lago 1969*b*, p. 51; Ritter 1974, p. 106). However, because there is an unusually serious lack of economic data from

this time span, it has been difficult to specify precisely the degree of improvement. Available guesses focus on the annual rate of GNP growth in current prices. Estimates range from 12% annually, by Boti, for 1959–60 to 6% annually, by Effimov, for 1959–61 (see Mesa-Lago 1969*b*, p. 50). Noyola (1961) provides estimates of 10.5% a year for both 1959 and 1960. However, Pazos (1962) argues that these figures are too high, proposing a 1959 figure of from 5% to 6%.

Recalling the explanation of the level change for the energy consumption curve (see the discussion of fig. 5), the estimated energy consumption growth rate for 1959 would be 13.3% (from eq. [4], $b_1 + [b_2 - b_3] = .076 + [.089 - .032] = .076 + .057 = .133$). If this prediction were corrected downward by 4.1% as done earlier, then the estimated 1959 GNP growth rate, in constant prices, = 9.2% (adding the 1.1% inflation figure yields a 1959 GNP growth estimate, in current prices, of 10.3%, which is almost exactly Noyola's estimate). But, again, a more accurate growth picture probably comes from the per capita energy consumption equation prediction of the real per capita GNP growth. In this case, the 1959 estimate for the real per capita GNP growth rate = 7.0% (from eq. [5], $b_1 + b_2 - b_3 -$ constant error $= .052 + .088 - .028 - .042 = .070$). Hence, this ITS formulation of the energy consumption trend is correctly detecting the revolutionary high point in economic growth postulated by careful observers of the Cuban economy. Moreover, the fullest short-run revolutionary effect is captured when the first postintervention observation is 1959, rather than a later year; therefore, the initial model specification for X_2 (and X_3) in equations (4) and (5) is sustained.[10]

THEORETICAL IMPLICATIONS

These findings support none of the rival models—Conservative, Marxist, and Thermidorian—developed originally. The rejection of these three models, all of which share the assumption of short-run revolutionary costs, implies that the initial economic effects of revolutions, at least in the Cuban case, may not be as harmful as generally supposed. Rather, with regard to economic growth, short-run benefits and long-run costs appear to follow revolution. Metaphorically, the pattern might be labeled "euphoric," with elation replaced by gloom. For Cuba in 1959, the per capita real GNP

[10] In altering the point of intervention (for X_2 and X_3, year 1 = 1959, 1960, 1961, 1962, or 1963), one observes that b_2 declines steadily and ceases to be statistically significant for year 1 = 1962 or 1963. In contrast, the estimates for b_1 and b_3 remain very stable in the face of these manipulations. This stability heightens confidence in the reliability of the estimates of the per capita energy consumption rates for the pre- and postperiods, for it indicates that they are not merely a product of the particular starting and stopping points in eq. (5), i.e., 1949–58, 1959–74. (This same stability of b_1 and b_3 is observed when non–per capita energy consumption is so analyzed; likewise, the estimate for b_2 declines in a similar way, ceasing to be statistically significant if year 1 = 1962 or 1963.)

growth rate was 7.0%, by my estimate. This preliminary production gain came largely from the increased demand made possible by income redistribution and from the greater use of already existing resources. However, these economic advances were shortly negated by trade difficulties, the scarcity of managers and materials, and administrative error. In fact, looking across the entire Castro period, I estimate the annual rate of real per capita GNP growth at -1.8%, compared with 1.0% for the prerevolutionary era.

Nevertheless, even granting that the economic growth rate has fallen during the revolutionary years, the inference that Castro's policies are responsible for the decline is hasty. While the Cuban government obviously directs the Cuban economy, there are external influences over which it has little control. Of overwhelming importance here is U.S. policy. Specifically, what has been the impact of the U.S. embargo? In the case at hand, the imposition of the embargo is almost coincident with the revolution and so may share responsibility for the drop in economic output. Ideally, revolutionary effects should be tested in the absence of an embargo.

This ideal circumstance prevailed during the first year of the revolution. Throughout 1959, international trade relations followed the prerevolutionary pattern (serious U.S. economic reprisals began in 1960 with reduction of the sugar quota in July and the imposition of a partial embargo in October). Thus, a partial test of revolutionary effects, disentangled from the embargo, is found in the short-run effect coefficient, b_2, of eq. (5). As discussed, in this first year growth was significantly greater than for 1958. Also, sugar production held steady, with no indication of a drop from the previous year ($b_2 = .031$, $t = .146$). However, the embargo soon led to serious shortages of raw materials and replacement parts for industry, and output slowed. Hence, the temptation is to attribute the long-run production decrease to persisting effects of the embargo. But, at about the same time the embargo was taking hold, important internal changes in Cuba were occurring, for example, the transition to a socialist state was being completed, and the Soviet Union was assuming a major role. Therefore, by the 1960–61 period, major variables potentially influencing growth are once again confounded. Nevertheless, the brief 1959 experiment hints at the revolutionary possibilities for positive economic growth. Without a doubt, the subsequent decline was at least partially caused by the far-reaching economic reorientation that the U.S. sanctions required and cannot fairly be ascribed solely to the Castro government.

To what extent can one expect the Cuban findings to hold for other revolutions? Is the impact on Cuba different from the impact on Russia? Or France? Perhaps, as the Cuban case suggests, the consequences of revolution vary according to the nation's size and international dependency. It may be that its effects are associated with the initial economic development level of the country or with the social origins of the revolutionary

leadership. To the degree that such conditions pertain, unqualified inferences from the Cuban experience to the universe of revolutions are unwarranted. Clearly, in building a theory of effects, more revolutions must be studied, and the implication is that statistical interactions must be seriously considered. Further, investigation of other revolutions promises longer time series, and the opportunity to include intermediate- as well as short- and long-run effects in an ITS analysis.

Revolutionary effects may be conditioned by the dependent variables themselves, as well as by certain independent variables. In my investigation, I focused on the impact of revolution on economic variables. But it is possible, even likely, that revolution tends to influence social and political variables differently. Thus, while additional research might provide further support for a "euphoric" model with regard to economic growth, that model might not hold in other areas of society. One of the three models posed at the outset might actually apply. Or yet another formation could be relevant. For example, with regard to social variables, the typical pattern may be a sort of "mobilization" model (e.g., educational attainment levels are at first unaffected by the revolution [$b_2 = 0$] but then commence a steady rise [$b_3 > 0$]).

Entertaining the idea of different models for different sets of dependent variables throws into sharp relief the complexity of the perennial question of social philosophers, "Is revolution worthwhile?" Even if this question could somehow be answered for each sector—the economic, the social, and the political—it would persist at a higher level. For instance, what would be the objective criteria for determining that the social benefits of revolution outweighed the political costs? Premier Castro, in a 1971 speech in Chile, declared that "the accomplishments of a revolution are not to be measured only in stones; they are not to be measured only in factories. They *are* to be measured in these, but essentially they ought to be measured in moral and human terms" (as quoted in Matthews 1975, p. 439). I share Castro's concern that all the outcomes of revolution, the moral along with the material, be measured. But evaluating the worth of a revolution ultimately reaches beyond measurement of the relevant variables. Even after all the various revolutionary outcomes are accurately recorded, it remains to be asked, "Which do I prefer?" and "How much do I prefer them?" These questions, of course, force us to look beyond the methodology of this paper.

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Suicide, Motor Vehicle Fatalities, and the Mass Media: Evidence toward a Theory of Suggestion[1]

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Tarde and other classical sociologists paid a great deal of attention to the concepts of imitation and suggestion, but these concepts have been virtually ignored in modern sociology. This paper presents new findings indicating that imitation and suggestion have a powerful impact on social behavior: Three days after a publicized suicide, automobile fatalities increase by 31%. The more the suicide is publicized, the more the automobile fatalies increase. The age of the drivers is significantly correlated with the age of the person described in the suicide story. Single-car accidents increase more than other types just after the publicized suicide. After presentation of these and related findings, the paper discusses some ways in which the concepts of suggestion and imitation can be incorporated into sociological theory.

Contemporary sociologists have largely neglected the social effects of suggestion, modeling, and imitation.[2] This neglect is somewhat puzzling in view

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[2] A small part of this paper has been briefly summarized in a two-page report in *Science* (Phillips 1977*a*). The terms "imitate," "suggest," and "model" will be defined throughout this paper as they are in *Webster's Unabridged Dictionary:* (1) imitate: to follow as a pattern, model, or example; (2) suggest: to mention as a possibility, to offer as an idea for consideration; (3) model: a person or thing regarded as worthy of imitation. Although these concepts have been neglected in contemporary sociology, this is of course not the case in classical sociological theory (see, e.g., Tarde 1903; Le Bon 1895; Mead 1934).

of the widespread interest in suggestion and modeling displayed outside sociology. Each year the U.S. advertising industry spends more than $23 billion on the working assumption that commercial behavior is influenced by suggestion, modeling, and imitation. (For information on the amount spent on advertising, see Sandage and Fryburger [1975, p. 10]; for reviews documenting the effects of advertising, see Dirksen and Kroeger [1973]; Michman and Jugenheimer [1976]; Sandage and Fryburger [1975]; for general reviews of factors affecting consumer behavior, see Engel, Kollat, and Blackwell [1973]; Cohen [1972]; Britt [1970]; Jacoby [1976].) The literature on these topics is estimated to include at least 5,000 articles and books (Jacoby 1976). The effect of imitation, suggestion, and modeling has also been extensively documented in a noncommercial context—most notably by psychologists studying the effects of the mass media (especially television) on violence (Klapper 1960; Berkowitz 1962; Flanders 1968; Goranson 1969; Singer 1971; Bandura 1973; Comstock and Fisher 1975; Comstock 1975; Comstock and Lindsey 1975; Liebert and Schwartzberg 1977). To date, there have been more than 2,300 studies on this topic, the most widely known being the Surgeon General's report on the impact of televised violence (1972).

Much of the literature on this topic consists of laboratory-based studies of children or youths. On the basis of these studies, many investigators agree that certain types of television and film programs help to stimulate violence in children in the laboratory (see references above). But investigators disagree strongly on whether these laboratory studies imply that the mass media have any serious effect on adult violence in the real world.[3]

The purpose of this paper is to provide sociological evidence indicating a previously unsuspected, real-world effect of suggestion on one kind of adult violence. In view of the results to be presented, it may be valuable to consider whether the concepts of suggestion, modeling, and imitation can be usefully incorporated into sociological theory. Does the use of these concepts sensitize us to theoretical issues previously neglected and provide us with answers to problems previously unsolved? This question will be addressed after presentation of the data.

DESCRIPTION OF THE STUDY

The study to be presented can be best understood in the light of an earlier paper (Phillips 1974). In that paper I showed that (1) the national level of suicides increases significantly for a brief period after a suicide story is

[3] For a discussion of this issue, see Comstock (1975, pp. 36–40). Of course, the preceding discussion should not be taken to imply that social psychological laboratory studies are not valuable, only that the relatively precise causal analysis made possible by such studies needs to be supplemented by other types of study which allow one to determine whether laboratory studies can be generalized to the "real world."

publicized by the newspapers (for example, after Marilyn Monroe committed suicide, the U.S. suicide rate temporarily increased by 12%); (2) this increase in suicides occurs only after the suicide story is published; (3) the more publicity given to the suicide story, the more the national level of suicides increases; and (4) the increase in suicides occurs primarily in the geographic areas where the suicide story is publicized. The increase in suicides after a publicized suicide story remains after one corrects for the effects of random, seasonal, and yearly fluctuations on the suicide level. After testing several alternative explanations of these findings, I concluded that suicides increase after a publicized suicide story because some persons are imitating the publicized suicide. The phenomenon thus discovered was labeled "the Werther effect," after Goethe's literary hero who committed suicide, and was subsequently imitated.

As long as one can assume that the Werther effect is an isolated sociological phenomenon, one need not consider whether concepts like suggestion should be added to the body of sociological theory. If, however, the effect turns out to be not an isolated phenomenon, but part of a general category of similar phenomena, it may be useful for sociologists to pay more serious attention to concepts like suggestion.

In this paper I will address a host of questions left unanswered in the earlier study. In addressing these questions, I will show (among other things) that (1) suggestion affects behavior which was previously thought to be unaffected by it; (2) different types of publicized stories exert different types of effect; (3) publicized stories affect some types of people more than others.

THE EFFECT OF SUGGESTION ON DRIVERS IN MOTOR VEHICLE ACCIDENTS

Motor vehicle accidents are the fifth leading cause of death in our society (U.S. Public Health Service 1974). Although the evidence is far from conclusive, many researchers have conjectured that some motor vehicle accidents have a suicidal component.[4] If this is so, motor vehicle accidents

[4] For example, see Litman and Tabachnick 1967; Porterfield 1960; MacDonald 1964; Tabachnick et al. 1966, 1973; Preston 1964; Preston and Harris 1965; Conger et al. 1957, 1959; Brown and Birdie 1960; Tillman and Hobbs 1949; Selzer 1969; Finch and Smith 1970; Selzer and Payne 1962; Crancer and Quiring 1970. These investigations are based largely on psychiatric or psychological studies of small, nonrandom samples restricted to very small geographic areas. In a typical study of this type, the researcher seeks to find a similarity between the psychological profile of the auto accident victim and the psychological profile of the suicide. In some studies, a similarity is found (e.g., Tabachnick et al. 1966); in others, sometimes by the same investigators, no similarity is found (Tabachnick et al. 1973; Preston 1964). For a more complete review of the contradictions and methodological problems inherent in this literature, see Phillips (1977*a*).

should behave just like suicides; that is, they should increase very markedly just after publicized stories about suicides. I will now show that motor vehicle accidents do indeed behave in this fashion.

Method of Analysis

The mortality data to be examined consist of tables of daily motor vehicle fatalities. Four states publish such information (California, Michigan, Texas, and South Carolina),[5] but only the largest of the four (California) will be examined here. In 1970, the midpoint of the period to be studied, California accounted for 9.81% of all U.S. motor vehicle fatalities (U.S. Public Health Service 1974). The remaining three states will be examined in a subsequent paper.

An exhaustive list of front-page suicide stories was generated from the reference libraries of the *Los Angeles Times* and the *San Francisco Chronicle*.[6] These are, respectively, the largest newspapers in the Los Angeles and the San Francisco Standard Metropolitan Statistical Areas, the two largest Standard Metropolitan Statistical Areas in the state. Three additional large California papers will also be investigated below.

If front-page California suicide stories stimulate a rise in California motor vehicle fatalities, then this increase can be detected by a technique adapted from earlier work (Phillips and Feldman 1973; Phillips 1974; Phillips 1977*a*). The use of this technique can be illustrated by the case of Yukio Mishima, a Japanese author who killed himself amid great international publicity on Tuesday, November 24, 1970. In the "experimental period" consisting of the week after Mishima's death (November 24–November 30), there were 117 motor vehicle fatalities in California.[7] One can determine whether this is an abnormally large number of deaths[8] in the experimental

[5] Personal communication from Grace Hazzard and Reuben Harding, Statistical Information Staff, National Highway Traffic Safety Administration.

[6] In keeping with previous practice (Phillips 1974), only front-page (not inside-page) newspaper stories have been examined. One might also wish to examine the effect of suicide stories publicized on television. Unfortunately, it is very difficult to compile a systematic list of stories publicized by the 52 television stations in California in 1970 (Television Digest 1970) because these stations do not in general keep an index of the stories they have covered through the years.

[7] At the beginning of this study the "experimental period" was arbitrarily set at one week instead of, say, a month. This choice was made because it was assumed that the effect (if any) of a suicide story on motor vehicle deaths would last about a week. In general, for all the suicide stories studied, the experimental week starts with the earliest date on which publicity might be devoted to the suicide story. Consequently, the "experimental week" starts on the day of the suicide, not on the day when the suicide story appears in the newspaper (which is often a day after the suicide occurs). Following the above procedure allows one to detect the early effects of television or radio publicity which may be devoted to a suicide before it is described in the newspapers.

[8] Information on the daily number of California motor vehicle fatalities is obtained from California Highway Patrol, *Annual Report of Fatal and Injury Motor Vehicle*

period by comparing this number (117) with the number of deaths occurring in "control periods" in other years. These control periods are matched with the experimental period in several ways:

1. Month of the year. Since the experimental period falls in November, the control periods are chosen so as to fall in November also. Matching control periods and experimental periods with respect to month corrects for the effect of seasonal fluctuations on motor vehicle fatalities.

2. Day of the week. Motor vehicle fatalities vary markedly by day of the week;[9] thus, it is advisable to match control periods and experimental periods with respect to days of the week. In the present example, this means choosing one-week control periods which fall at the end of November *and* begin on Tuesday, just as the experimental period does. Matching control periods and experimental periods with respect to day of the week corrects for the effect of weekday fluctuations on motor vehicle fatalities.

3. Presence or absence of holiday weekends. Motor vehicle fatalities increase markedly during holiday weekends; hence, if the experimental period contains a holiday weekend, the control periods should also. Conversely, if the experimental period does not contain a holiday weekend, the control periods should not either. Matching control and experimental periods with respect to holiday weekends corrects for the effects of holiday weekends on motor vehicle fatalities.

For the period under analysis in this paper, 1966–73,[10] four control periods contain no publicized suicides and can be matched with the experimental period, Tuesday, November 24–Monday, November 30, 1970, in the ways described above.

The number of motor vehicle fatalities in each of these four control periods was calculated, and a regression line was fitted to these data. From this regression line, one would expect 98.88 motor vehicle fatalities in the experimental period, November 24–30, 1970, under the null hypothesis that publicized suicides have no effect on motor vehicle fatalities. There was a rise in motor vehicle deaths in the week after Mishima killed himself. The observed number of fatalities in this period (117) is greater than the number expected (98.88) ($P < .05$, one-tailed t-test).

Traffic Accidents (yearly issues). For the week after the publicized suicide, one might wish to examine fatality rates rather than the number of fatalities. Unfortunately, it is not possible to calculate weekly fatality rates because the weekly population figures needed for these calculations do not exist.

9 This is immediately apparent from inspection of daily fatality figures, as is the marked increase during holiday weekends mentioned in the same paragraph.

10 The period of analysis begins in 1966 because that is the earliest year for which the California Highway Patrol published daily motor vehicle accident fatality statistics. The period of analysis ends in 1973 because that was the last year before the Arab oil embargo drastically changed patterns of gasoline consumption and patterns of motor vehicle traffic.

In general, the procedure just described was used to estimate the effect of all front-page suicide stories in California for which suitable control periods were available.[11] Some suicide stories could not be investigated because, for example, their control periods contained suicide stories and hence could not be used.

Results of Analysis

Table 1 gives the number of California motor vehicle fatalities observed after each front-page suicide story, the number expected under the null hy-

TABLE 1

RISE IN MOTOR VEHICLE FATALITIES AFTER PUBLICIZED SUICIDES, CALIFORNIA 1966–73

Identity of Publicized Suicide	Date of Publicized Suicide	Observed No. of Motor Vehicle Deaths in the Week after the Suicide	Expected No. of Motor Vehicle Deaths in the Week after the Suicide†	% Rise in Motor Vehicle Deaths 100 ([Observed−Expected]/Expected)
A. Korbel, winemaker	4-21-1966	96	89.67	7.06
S. Youngren, union leader	5-17-1966	79	80.40	−1.74
Lo Jui-Ching, army leader	1-19-1967	81	80.92	.10
J. Hughes, businessman*	2-23-1967			
E. Joe, student*	2-23-1967	102	78.90	29.28
S. Abshear, student*	2-25-1967			
A. Amer, Egyptian general	9-14-1967	99	101.20	−2.17
M. Berg, L.A. policeman	9-23-1967	105	100.70	4.27
V. Janko, Czech general	3-14-1968	88	84.25	4.45
F. Chegwin, mass murderer	8-8-1968	100	89.40	11.86
H. Luedke, NATO admiral*	11-10-1968	120	89.14	34.62
V. Latham, housewife*	11-10-1968			
J. Palach, Czech student	1-16-1969	102	78.08	30.64
Y. Mishima, author	11-24-1970	117	98.88	18.33
J. Mattison, prisoner	2-25-1971	90	85.61	5.13
B. Pollack, orchestra leader	6-7-1971	82	102.70	−20.16
G. Giffe, hijacker	10-4-1971	104	97.50	6.67
G. Logan, mass murderer	11-26-1971	89	82.57	7.79
J. Van Praag, psychologist	3-7-1972	103	75.67	36.12
M. Oufkir, defense minister	8-17-1972	101	83.89	20.40
M. Brody, millionaire	1-26-1973	65	81.00	−19.75
E. Brudno, ex-P.O.W	6-3-1973	98	102.14	−4.05
W. Inge, playwright	6-10-1973	100	88.03	13.60

* Suicides occurring within one week of each other and therefore treated as one story.

† See text for data sources and for methods of calculating expected number of deaths.

[11] When the number of deaths expected in the experimental period could be estimated from a regression line interpolated between control periods, a minimum of two control periods was required. When the expected number of deaths had to be extrapolated from control periods, a more conservative procedure was followed: (1) A minimum of three control periods was required. (2) A suicide story was not examined if one needed to extrapolate the regression line for more than one year. Researchers have often recommended more conservative estimation procedures when estimates must be based on extrapolated data.

pothesis that suicide stories have no effect on motor vehicle fatalities, and the difference between the observed and the expected numbers of fatalities. On the average, the number of motor vehicle fatalities increased by 9.12% in the week after each suicide story ($P = .011$, t-test, one-tail).[12]

Timing of Motor Vehicle Fatalities in Relation to the Publicized Suicide

If the observed rise in motor vehicle fatalities is caused by publicized suicide stories, then this rise should occur only after each story appears. Table 2 enables us to examine the day-by-day fluctuation of motor vehicle fatalities for a two-week period, beginning two days before the publicized suicide and ending 11 days afterward.[13] It is evident that motor vehicle fatalities do not rise immediately before the publicized suicide and do rise rather steeply

TABLE 2

DAILY FLUCTUATION IN MOTOR VEHICLE FATALITIES FOR A TWO-WEEK PERIOD BEFORE, DURING, AND AFTER PUBLICIZED SUICIDES

No. of Days before or after Publicized Suicides	No. of Deaths Observed	No. of Deaths Expected	Percentage Fluctuation: 100 ([Observed − Expected]/ Expected)
2 days before	237	247.038	−4.06
1 day before	264	268.714	−1.75
Day of publicized suicide	260	247.716	+4.96
1 day after	269	254.037	+5.89
2 days after	313	300.624	+4.12
3 days after	338	257.440	+31.29
4 days after	264	244.205	+8.11
5 days after	244	236.771	+3.05
6 days after	233	225.477	+3.34
7 days after	226	236.694	−4.52
8 days after	304	278.341	+9.22
9 days after	329	315.091	+4.41
10 days after	288	291.293	−1.13
11 days after	240	236.073	+1.66

NOTE.—For the method of calculating the number of deaths expected, see text.

12 The statistical techniques used in this paper are plausible but arbitrary, in the sense that other, equally plausible techniques might have been substituted for the ones actually used. The results (table 1) remain statistically significant even if these alternative techniques are used: (1) When these results are analyzed by the Walsh test, $P = .010$, one-tailed test. (2) The expected number of deaths after a story might plausibly be estimated by averaging the deaths in the control periods rather than by fitting these deaths to a regression line. In this case, auto fatalities rise by an average of 7.15%, $P = .019$, one-tailed t-test; $P = .016$, one-tailed Walsh test. In view of these findings, it is difficult to conclude that the results in table 1 are an artifact of the statistical technique used to analyze them.

13 The techniques described earlier for calculating the expected number of deaths in any given week are adapted for calculating the expected number of deaths on any given day. These techniques can be illustrated by the case of Mishima's suicide on Tuesday, No-

immediately after the suicide. The most noteworthy feature of this table is the brief but very strong increase in motor vehicle fatalities three days after the publicized suicide, at which time motor vehicle fatalities are 31% (and 80 deaths) higher than expected. The data in table 2 are graphed in figure 1.

I have now shown that motor vehicle accident fatalities increase significantly after publicized suicide stories, and not before. In the next section I will provide additional evidence suggesting a causal link between suicide stories and a rise in motor vehicle fatalities.

Relationship between Amount of Publicity Devoted to Each Suicide Story and Increase in Fatalities after Each Story

If motor vehicle fatalities rise because of suicides publicized by the newspapers, two additional predictions should hold true: (1) an index of the

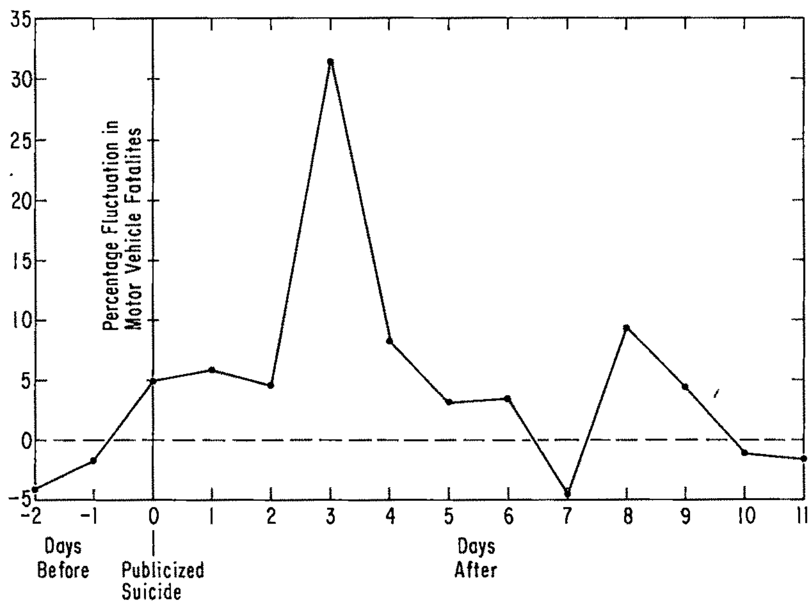

FIG. 1.—Daily fluctuation in motor vehicle accident fatalities for a two-week period before, during, and after publicized suicides.

vember 24, 1970. The expected number of deaths on this day is calculated by fitting a regression line to the four control days: Tuesday, November 22, 1966; Tuesday, November 21, 1967; Tuesday, November 26, 1968; and Tuesday, November 21, 1972. A similar regression period is used to calculate the expected number of deaths on the day of each of the publicized suicides. Table 2 indicates that the sum of these 20 expected numbers is 247.716, the total number of deaths expected on the day of the publicized suicides. The remaining expected numbers in table 2 were calculated in a similar fashion.

newspaper publicity devoted to a story should correlate positively with the rise in motor vehicle fatalities after that story; (2) the more accurate this index of publicity, the more highly it should correlate with the rise in motor vehicle fatalities.

A much-publicized story is one covered by many newspapers with a large combined circulation; a little-publicized story is one covered by few newspapers with a small combined circulation. Ideally, one should examine all 150 or so California daily newspapers to determine the total circulation achieved by a particular story. In practice, the most one can readily achieve is an examination of the five largest papers, which accounted for 41% of the total 1970 daily newspaper circulation in California.[14]

The publicity devoted to a story by these five newspapers (table 3, col. 2) can be used as an index of the total amount of publicity devoted by all newspapers. For any given story, the value of this index was calculated from

TABLE 3

RELATIONSHIP BETWEEN THE AMOUNT OF PUBLICITY DEVOTED BY THE FIVE BIGGEST CALIFORNIA NEWSPAPERS TO EACH SUICIDE STORY AND THE RISE IN MOTOR VEHICLE FATALITIES AFTER EACH STORY

Name of Publicized Suicide*	Total Circulation of Newspapers Covering the Story	Rise in Motor Vehicle Fatalities in the Week after the Story (%)
Palach	2,627,084	+30.64
Logan	1,858,095	+7.79
Lo Jui-Ching	1,799,279	+.10
Van Praag	1,673,972	+36.12
Mishima	1,641,766	+18.33
Luedke and Latham	1,578,018	+34.62
Hughes, Joe, and Abshear	1,414,326	+29.28
Giffè	1,376,928	+6.67
Janko	1,348,430	+4.45
Oufkir	1,022,359	+20.40
Inge	1,004,908	+13.60
Pollack	966,293	−20.16
Brudno	951,263	−4.05
Chegwin	856,621	+11.86
Amer	847,869	−2.17
Berg	847,869	+4.27
Korbel	750,000	+7.06
Youngren	750,000	−1.74
Mattison	478,704	+5.13
Brody	458,163	−19.75

* To facilitate analysis, suicide stories have been listed in the order of the amount of publicity received by each story.

14 Information on newspaper circulation figures and on the procedures for ensuring that these figures are accurate is provided by *Ayer Directory of Publications* (yearly volumes) and by the Audit Bureau of Circulations, Chicago.

$$\sum_{i=1}^{5} x_i y_i ,$$

where x_i is the circulation of newspaper i at the time of the story, and y_i is the number of days that newspaper carried the story on page 1. This five-newspaper publicity index correlates positively ($r = .59$, $P < .005$) with the change in motor vehicle fatalities after each story (table 3, col. 3). On the average, fatalities increased by 18.84% after stories receiving more than the median amount of publicity and decreased by a statistically insignificant amount, 0.60%, after stories receiving less than the median amount of circulation.

In table 3, a five-newspaper circulation index was used. In general, the more newspapers included in a circulation index, the more highly it should correlate with the rise in motor vehicle fatalities after publicized suicide stories. In all, five indexes have been constructed. These range from index 1 (constructed from circulation data for the single largest California newspaper) to index 5 (constructed from circulation data for the five largest California newspapers). Each index is correlated with the fluctuation of motor vehicle fatalities as follows: index 1, $r = .196$; index 2, $r = .300$; index 3, $r = .478$; index 4, $r = .576$; and index 5, $r = .594$. As predicted, the more newspapers included in the index of publicity, the higher the correlation with motor vehicle fatalities. These five indexes would be ranked in the predicted order by chance .0083 (1/120) of the time.

We have now seen that motor vehicle accidents increase sharply just after publicized suicide stories. We have also seen that the more publicity devoted to a suicide story, the more motor vehicle fatalities rise thereafter. The more precisely one measures publicity, the more strongly it correlates with the rise in motor vehicle fatalities.

Some Possible Alternative Explanations of the First Set of Findings

The findings I have presented above are consistent with the hypothesis that publicized suicide stories help to stimulate a brief increase in motor vehicle fatalities. The results cannot be ascribed to the effect of weekday or monthly fluctuations in motor vehicle fatalities, to holiday weekends, or to yearly linear trends, because these effects were corrected for in the selection and treatment of the control periods with which the experimental periods are compared. Furthermore, because the results are statistically significant, they cannot plausibly be ascribed to chance fluctuations in the data.

The hypothesis that a prior change in social conditions caused both the publicized suicide and the subsequent rise in motor vehicle fatalities is implausible for two reasons: (1) if such conditions created both a wave of motor vehicle fatalities and the front-page suicide, the suicide would be

expected to occur during the wave of fatalities rather than before it; (2) the prior-conditions explanation implies that there is no link between the characteristics of front-page news stories and the increase in motor vehicle fatalities. If no such link exists, it is difficult to explain the observed association between the publicity given to a suicide and the rise in fatality levels thereafter. Additional, minor alternative explanations have been examined elsewhere (Phillips 1977*b*).

The best available explanation of the rise in fatalities is that it is caused by the suggestive effect of suicide stories. The precise nature of this effect will be much better understood after one examines the characteristics of the accidents and the characteristics of the drivers dying in those accidents.

DETAILED ANALYSIS OF TYPES OF ACCIDENTS OCCURRING JUST AFTER SUICIDE STORIES

In this section I will attempt to answer questions like the following: Do stories about suicide trigger single-car accidents rather than multiple-car accidents? Do stories about suicide combined with murder trigger multiple-car accidents (this being a method by which a driver can kill others as well as himself)? Is the driver who dies after a suicide story similar to the person described in that suicide story? In seeking to answer questions like these one gains insight into the social and psychological processes producing the observed increase in motor vehicle fatalities. In addition one gains a more general understanding of the processes of imitation, suggestion, and modeling.

Selection of Accidents to Be Examined

When one attempts to determine the characteristics of accidents occurring after suicide stories, it is inefficient to examine all accidents that occurred throughout the state of California. One would prefer, instead, to include only those adult decedents who were most likely to have been exposed to the suicide stories under study. These stories were all publicized in the Los Angeles or San Francisco Standard Metropolitan Statistical Area and were not necessarily publicized elsewhere in California. Consequently, I will now restrict attention to adult Californians who resided and/or died in the Los Angeles or San Francisco Standard Metropolitan Statistical Area in 1969–73. Mexican-Americans will be excluded from the analysis because surveys show that this ethnic group is much less likely than others to read the largest California newspapers.[15]

[15] The published tabulations used in the first part of this paper pertain to all Californians for the period from 1966 to 1973. They are not subclassified by age, race, geographic location in California, type of accident, etc. With the computerized death certificates used in the second part, these subclassifications are possible but, in some cases, only for 1969 and thereafter. Consequently, the analysis in this part of the study must

In examining accidents following suicide stories, one would prefer to focus attention on the "suicidal" accidents which were triggered by the suicide story rather than the "conventional" accidents which would have occurred anyway, even in the absence of a suicide story. Unfortunately, the available data do not allow us to separate suicidal from conventional accidents; the best we can do is examine all accidents in the time period which contains the largest possible ratio of suicidal accidents to conventional ones.

The method for selecting this time period can best be understood if we refer again to figure 1, which shows the fluctuation of motor vehicle fatalities before and after publicized suicide stories. The dotted line in the graph indicates the level of motor vehicle fatalities expected if fatalities were unaffected by suicide stories. Accidents indicated below the dotted line are presumably conventional motor vehicle fatalities which would be expected to occur even in the absence of suicide stories. The excess fatalities indicated above the dotted line of the graph presumably include some suicidal accidents which were triggered by the publicized suicide stories. It is evident from this graph that the largest number of excess fatalities occurs on the third day after each publicized suicide. Hence, this day should contain the largest proportion of suicidal automobile fatalities. Thus, I have chosen the third day after each story as the experimental period in this part of the study.

The control period to be used in this part of the study consists of the seventh day after the experimental period and the seventh day before the experimental period. Thus, the control period always falls on the same day of the week as the experimental period and, in general, control periods and experimental periods will also be matched with respect to month and year.[16] This method of matching experimental and control periods allows us to correct for the effect of weekday, monthly, and yearly fluctuations in the characteristics of motor vehicle fatalities. Therefore, any differences found

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be restricted to 1969–73 instead of the longer period used earlier. In general it should be stressed that the data base for this part of the paper is a subset of that employed above. The following definitions have been used in this part of the study: A Californian is someone who resided and died in California. An adult is someone aged 15 or over. A Mexican-American is defined as an American who was born in Mexico. One might prefer to broaden this definition to include (say) all Spanish-speaking Americans, but unfortunately the information needed to apply a broader definition is not available in the computerized death certificates used in this study. The *Los Angeles Times* readership study showed that only 13.5% of adult Mexican-Americans in Los Angeles read the weekday *Los Angeles Times;* in contrast, 36.15% of all other adults read the *Los Angeles Times.*

[16] The method for choosing control periods in the first part of this study can not be used in this part of the study. The method used in the first part of the study is feasible when a relatively large span of years is examined (1966–73) but not for the shorter span of years covered in this current section of the paper.

between the two periods will not be attributable to weekday, monthly, or yearly fluctuations in the data. In all, 169 deaths will be examined in this part of the study—62 deaths in the experimental period and 107 deaths in the control period. Because the control period consists of twice as many days as the experimental period, it is expected to have more deaths.

I will now examine the characteristics of "experimental group" accidents (which follow suicide stories) and the characteristics of "control group" accidents (which occur at other times).

Are Single-Vehicle Accidents Abnormally Frequent Just after Suicide Stories?

Researchers have often supposed that suicidal accidents are more likely to be single-vehicle accidents than to be other types.[17] If this is so, the ratio of single-vehicle accident deaths to other types should be larger in the experimental period than in the control period. This prediction is confirmed by the data in table 4: the ratio of deaths from single-vehicle accidents to deaths from other types is 29:33 = .88 in the experimental period, while the ratio is much smaller in the control period (32:75 = .43). The difference between these two ratios is significant at .0213 (hypergeometric, one-tail); hence deaths from single-vehicle accidents increase more than other types just after a publicized suicide story.[18]

TABLE 4

RELATIVE FREQUENCY OF DEATHS FROM SINGLE-VEHICLE ACCIDENTS JUST AFTER SUICIDE STORIES ARE PUBLICIZED—EXPERIMENTAL PERIOD AND OTHER TIMES (Control Period)

	Deaths from Single-Vehicle Accidents	Deaths from Other Motor Vehicle Accidents
Experimental period	29	33
Control period	32	75

NOTE.—"Single-vehicle accidents" are defined in n. 17.

[17] The literature on this topic was reviewed earlier in this paper (in n. 4 above). A single-vehicle accident is defined here as an accident coded as E815 or E816 in the Eighth International Classification of Causes of Death; E815 and E816 comprise all motor vehicle collision traffic accidents not involving a train, streetcar, another motor vehicle, another vehicle, or a pedestrian.

[18] This application of the hypergeometric is discussed by Hoel (1965) and even more specifically by Mosteller, Rourke, and Thomas (1970).

Do the Drivers in Single-Vehicle Accidents Identify with the Person in the Suicide Story?

If the drivers in single-vehicle accidents have identified with the person in a suicide story, then suicide stories about young persons should tend to induce single-vehicle crashes with young drivers. Conversely, suicide stories about older persons should induce single-vehicle crashes with older drivers. These predictions are supported by the available data: there is a positive correlation ($r = .4634$, $P = .023$, t-test, one-tailed, 16 df) between the age of the publicized suicide and the age of drivers in single-vehicle crashes which occur just after the publicized suicide.[19]

The findings presented above indicate that suicide stories may lead to relatively many single-vehicle crashes but relatively few multiple-vehicle crashes. Hence, it is probable that only a small proportion of multiple-vehicle crashes are caused by suicidal persons who have identified with the publicized suicide. Thus, one would not expect to find a strong correlation between the age of the publicized suicide and the age of drivers in *multiple*-car crashes which occur just after the publicized suicide. As predicted, r is small ($= .0655$, $P = .416$, t-test, one-tailed, 10 df). A similarly small, insignificant correlation is found when one compares the age of the publicized suicide with the age of *non*drivers who die just after the suicide story ($r = .2109$, $P = .146$, t-test, one-tailed, 25 df).[20]

Do Different Types of Suicide Stories Trigger Different Types of Motor Vehicle Accidents?

Two major types of suicide story can be distinguished: (1) pure suicide stories, in which a person kills himself and no one else, and (2) murder-suicide stories, in which a person kills others as well as himself. One might expect that pure suicide stories would tend to trigger single-car accidents in which the driver dies, while murder-suicide stories would tend to induce multiple-car accidents in which passengers die. Table 5 shows the relative frequency with which deaths from different types of accident occur after each type of suicide story. Although the numbers in this table are quite

[19] Some of the assumptions necessary for testing the statistical significance of this correlation may not hold. Thus, one might prefer an alternative statistic (e.g., the Spearman rank-correlation coefficient) which requires less stringent assumptions. The Spearman rank correlation $= .444$ (corrected for ties). This figure is significant at $< .05$, one-tailed. It would be interesting to determine whether the race and sex of the publicized suicide are correlated with the race and sex of drivers dying just after the suicide story. Unfortunately, one cannot make this determination because publicized suicides hardly vary with respect to race and sex—almost all are white males.

[20] As one would expect, there is no significant correlation between the age of the publicized suicide and the age of persons dying in the control period.

TABLE 5

CHARACTERISTICS OF MOTOR VEHICLE ACCIDENT DEATHS FOLLOWING SUICIDE AND MURDER-SUICIDE STORIES

	Driver Deaths from Single-Vehicle Accidents (1)	Driver Deaths from Multiple-Vehicle Accidents (2)	Passenger Deaths from Single-Vehicle Accidents (3)	Passenger Deaths from Multiple-Vehicle Accidents (4)	Pedestrian Deaths from Motor Vehicle Accidents (5)	Deaths from Other (Miscellaneous) Accidents (6)
Suicide stories......	15	12	5	2	5	5
Murder-suicide stories..........	3	5	4	6	0	0

NOTE.—The deaths in each column correspond to the following categories in the Eighth International Classification of Causes of Death: 8152, 8162, 8150, 8160 (col. 1); 8102, 8112, 8122, 8132, 8100, 8110, 8120, 8103 (col. 2); 8151, 8161, 8153, 8163 (col. 3); 8101, 8111, 8121, 8131, 8103, 8113, 8123, 8133 (col. 4); 8147 (col. 5); 8128, 8129, 8169, 8181 (col. 6). The majority of cases in col. 6 involve residual categories 8129 and 8169. In these accidents the death certificate fails to specify the nature of the person killed (e.g., a driver, passenger, etc.).

small, the differences among them are so extreme that the results in table 5 are statistically significant. Table 5 shows that accident deaths following pure suicide stories are significantly different from accident deaths following murder-suicide stories ($\chi^2 = 14.67$, $P = .012$, 5 df). Multiple-vehicle accidents involving passenger deaths tend to be relatively frequent after murder-suicide stories and quite rare after pure suicide stories. Conversely, single-vehicle accidents involving driver deaths are very frequent after pure suicide stories and very rare after murder-suicide stories. These results are consistent with the notion that a pure suicide story stimulates some persons to commit suicide (but not murder), while murder-suicide stories stimulate some persons to commit murder and suicide.[21] (See also Phillips 1978 for further evidence on the effect of murder-suicide stories.)

Do the Crashes after Suicide Stories Occur Mainly in the Area Where the Suicide Stories Are Publicized?

If publicized suicide stories help to produce an increase in motor vehicle fatalities, the increase in fatalities should occur particularly in the places where the suicide story is publicized. This prediction will be tested after some terms are defined.

[21] The apparent overabundance of pedestrian deaths after pure suicide stories but not after murder-suicide stories may be explainable by the following speculative line of reasoning. A pedestrian can imitate a publicized suicide by stepping in front of a car and killing himself. But he cannot imitate a murder-suicide by stepping in front of a car, because he cannot commit murder by this action, only suicide. The results in table 5 must be interpreted with caution because the death certificate data do not indicate the number of persons dying in a given accident. Because of these limitations in the data, one cannot determine (for example) whether drivers dying immediately after a pure suicide story were alone at the time of their accidents.

For each suicide story, one can divide the Los Angeles and San Francisco region into two parts: (1) a "publicity area" consisting of the city or cities known to be receiving publicity on the story, and (2) a "nonpublicity area" consisting of the city not known to be receiving publicity on the story. Thus, for example, a story in the present study may be publicized in Los Angeles and not in San Francisco, in San Francisco and not in Los Angeles, or in both cities at once. For each story *s*, four statistics can be calculated: (1) $p(\text{exp})_s$, the number of deaths in the publicity area in the experimental period; (2) $p(\text{cont})_s$, the number of deaths in the publicity area in the control period; (3) $\bar{p}(\text{exp})_s$, the number of deaths in the nonpublicity area in the experimental period; and (4) $\bar{p}(\text{cont})_s$, the number of deaths in the nonpublicity area in the control period. Each of these four statistics can then be summed over all stories. For example, one can calculate $\Sigma p(\text{exp})_s$, the total number of deaths in the publicity area in the experimental period for all stories combined.

If motor vehicle fatalities increase mainly in the area where the suicide story is publicized, the ratio of deaths in the publicity area to deaths in the nonpublicity area should be greater for the experimental period than it is for the control period. This prediction is supported by the data in table 6. The two ratios in this table ($41{:}21 = 1.95$) and ($48{:}59 = .81$) are significantly different at .0058 (hypergeometric, one-tail). In other words, after a suicide story motor vehicle fatalities are abnormally frequent mainly in the area where the suicide story is publicized.

Are Crashes Just after Suicide Stories Unusually Lethal?

I will now examine one final difference between the conventional accidents in the control period and the suicidal accidents in the experimental period. In a suicidal accident the driver is consciously or unconsciously trying to die; hence, just before the crash the driver may well have his foot on the

TABLE 6

Number of Motor Vehicle Fatalities in the Publicity Area and the Nonpublicity Area during the Experimental Period and during the Control Period

	Publicity Area	Nonpublicity Area
Experimental period	41	21
Control period	48	59

Note.—For definition of the terms "publicity area" and "nonpublicity area," see text.

accelerator, not the brake. The victims[22] of such a suicidal crash should die very soon after the crash occurs, because the driver means the crash to be lethal. On the other hand, the victims of a conventional crash may well take longer to die after the crash occurs, because the driver in a conventional crash does not mean the crash to be lethal. In fact, the driver is trying to avoid the crash: he may well have his foot on the brake, not on the accelerator.

If this argument is correct, the average time between crash and death should be very short for the victims of the suicidal crashes in the experimental period. Conversely, the average time between crash and death should be relatively longer for the victims of the conventional crashes in the control period. These predictions are supported by the data in table 7. For the control period, the average time between crash and death, M_c, is 4.131 days; the average time for the experimental group, M_e, is only 1.016 days. The difference between these two figures, $M_c - M_e$, is significant at .0324 (one-

TABLE 7

NUMBER OF DAYS BETWEEN CRASH AND DEATH, JUST AFTER SUICIDE STORY (Experimental Period) AND AT OTHER TIMES (Control Period)

	No. Cases with x Days between Crash and Death	
No. Days between Crash and Death	For the Experimental Group	For the Control Group
0	51	79
1	5	6
2	1	0
3	0	2
4	0	1
5	0	2
6	1	4
7	0	2
8	1	1
9	1	1
10	1	0
11+	1	9

NOTE.—For convenience, the data are displayed in grouped form, but the averages discussed in the text (1.016 days for the experimental group and 4.131 days for the control group) were calculated from ungrouped data.

[22] In a crash involving a suicidal driver who is accompanied by passengers, the interval between crash and death should be very short for the driver *and* for the passengers. Hence, in this analysis I have examined not only driver deaths but nondriver deaths as well.

tailed t-test for difference between means, 122 df).[23] Thus, persons crashing after a suicide story die much more rapidly than persons crashing at other times.

SUMMARY

In this paper I have shown that: (1) motor vehicle accident fatalities increase markedly just after publicized suicide stories; (2) the increase in accident fatalities occurs only after the publicized story; (3) the more publicity given to the story, the greater the increase in motor vehicle fatalities; (4) the increase in fatalities occurs mainly in the area where the suicide story is publicized; (5) single-vehicle crashes increase more than other types just after suicide stories; (6) suicide stories about young persons tend to be followed by single-vehicle crashes involving young drivers; conversely, suicide stories about older persons tend to be followed by single-vehicle crashes involving older drivers; (7) stories about murder and suicide tend to be followed by multiple-vehicle crashes involving passenger deaths, while stories about suicide alone tend to be followed by single-vehicle crashes involving driver deaths. Thus, pure suicide stories and murder-suicide stories seem to trigger different types of crashes.

DISCUSSION

These findings indicate strongly that drivers of motor vehicles are affected by the processes of imitation, suggestion, and modeling. In view of these and previous findings on the impact of suggestion, it may be valuable to consider whether the concepts of suggestion, imitation, and modeling can be usefully incorporated into sociological theory. Does the use of these concepts sensitize us to theoretical issues previously neglected or overlooked? Can

[23] The difference, $M_c - M_e = 3.115$, cannot be ascribed to differences between the age distributions of the control group and the experimental group, because the difference $M_c - M_e$ actually increases from 3.115 to 3.314 after one corrects for age differences in the control and experimental groups. The statistical significance of the difference $M_c - M_e$ was calculated according to formulas 10.13.1–10.13.4, 10.17.1 in Hays (1963). Statistics used in these calculations are: $N_c = 107$ = number of items in the control group; $N_e = 62$ = number of items in the experimental group; estimated $\sigma^2 M_c = 2.616$; estimated $\sigma^2 M_e = .200$; $t = 1.8564$; df = 122 as calculated from formula 10.17.1 in Hays. Some of the assumptions required by the t-test may not be met by the data; hence, one might prefer an alternative statistical test with less stringent assumptions, e.g., the randomization test (Siegel 1956, pp. 152–56). The difference $M_c - M_e$ is significant at .031, randomization test, one-tail. The accidents in the control period and in the experimental period do not appear to differ significantly with respect to the other major variables coded on the computerized death certificate: race, sex, marital status, known involvement with alcohol or drugs.

one use concepts like suggestion to provide partial answers to problems previously unsolved? In considering these questions, I will illustrate the discussion with references to three very different areas of sociological theory: (1) behavioral decision theory; (2) reference group theory; and (3) the theory of anomie and its effects.[24]

Possible Usefulness of the Concepts of Suggestion, Imitation, and Modeling in Behavioral Decision Theory

In this theory, the individual is conceived of as a decision maker who analyzes a problem rationally, calculates the costs and benefits of various solutions, and, on the basis of these calculations, chooses an optimal solution (for reviews and general discussions of this topic, see Raiffa [1970]; Edwards and Tversky [1967]; Williams [1954]; Slovic, Fischoff, and Lichtenstein [1977]).

Decision theorists start by assuming a particular problem and then consider how the individual assesses possible solutions to that problem. By proceeding in this way, decision theorists ignore an important prior question: How does the individual choose the *problem* to be solved, before he even begins to concern himself with solutions? Out of the enormous range of problems plaguing any individual, what makes him single out one for attention rather than another?

Concepts like suggestion and modeling may provide a partial solution to this question. One may pay particular attention to a certain problem if that problem is made salient by a person who serves as a model. Thus, after Mrs. Nelson Rockefeller and Mrs. Gerald Ford discovered that they had breast cancer, breast cancer became a more salient problem for women across the United States. Similarly, after a fictional individual in a television commercial becomes aware of "bad breath," this problem may become salient for many viewers. Thus, suggestion and modeling may function as a kind of mediating mechanism, a kind of "mental switch" which leads an individual to become preoccupied with one problem rather than another.[25] After the individual has become concerned with a particular problem, then perhaps he may choose a solution to that problem according to the criteria of decision theory.

24 These three theories were chosen because: (1) each theory has generated a very large literature; (2) each is intended to cover a very wide range of behaviors; and (3) all three seem to overlook some basic questions which are related in some surprising and interesting ways (as will be indicated in the rest of this section).

25 The individual is not necessarily conceived of here as a helpless puppet, twitched one way or another by all-powerful suggestions and models. He may play an active role in the process of imitating, for example by seeking out favored models he wishes to imitate. This view of the individual as an active model seeker is consistent with a great deal of research on the effects of advertising (see, e.g., Sandage and Fryburger 1975).

A second, basic question ignored by decision theorists is: Why does an individual sometimes approach a problem in a deliberate, reasoning, and logical mode, while at other times he may approach the same problem in an impulsive, unreasoned, and illogical mode? In short, why does an individual behave according to the criteria of decision theory at some times and not at others?

Once again, suggestion, imitation, and modeling may provide a partial answer. An individual may adopt a decision-theoretic mode if that mode is displayed by his parents, teachers, or by other salient models. On the other hand, a non-decision-theoretic mode may be adopted if this alternative mode is made salient. Thus, for example, a child may solve a problem in an impulsive, unreasoned, and violent fashion if this mode is displayed on television. An adult may lash out violently and impulsively, killing others and himself, if this non-decision-theoretic mode is displayed by the media.

Here, as before, the processes of suggestion and imitation may serve as mediating mechanisms which help to switch an individual from one choice to another—in this case from a decision-theoretic mode to another mode of problem solving.

Possible Usefulness of the Concepts of Suggestion, Imitation, and Modeling in Reference Group Theory

In this theory, the individual is conceived of as influenced in behavior and attitude by his reference group (Schuman and Johnson 1976; Hyman and Singer 1968; Sherif and Sherif 1969; Merton and Rossi 1967; Merton 1967). As Jahoda has noted, reference-group theorists typically overlook a very central issue. They fail to "raise the question as to the conditions under which *different* reference groups become factors in determining opinions. As far as I know, this central question of reference group theory has not yet been tackled in research. As a rule, the concept is used as if the individual had only *one* group with whom he shared norms, namely the one deliberately built into the research design" (Jahoda 1972, my emphasis).

The concepts of suggestion, imitation, and modeling may provide a partial solution to the dilemma posed by Jahoda. One reference group may be chosen over another if a member of that reference group is publicized by the media. Thus, for example, the reference group "persons with troubles" may be chosen over others when the media publicize a story about a troubled person who commits suicide. On the other hand, the reference group "Christians" may become salient if the media carry a story about Billy Graham or the Pope, while still other reference groups may become salient on Father's Day, Mother's Day, George Washington's birthday, Jefferson Davis's birthday, or Martin Luther King's birthday.

Here again, as in our analysis of decision theory, publicity and suggestion may serve as mediating mechanisms helping to switch an individual from one choice to another—in this case from one reference group to another.

Possible Usefulness of the Concepts of Suggestion, Imitation, and Modeling in the Theory of Anomie and Its Effects

Many different behaviors have been thought to result from anomie. These range from various types of anomie-reducing social movements (Kornhauser 1959; Cantril 1963; Toch 1965; Klapp 1969; De Grazia 1948) to various types of deviant behavior, such as suicide, delinquency, alcoholism, drug addiction, and mental disorders (Durkheim 1951; Gibbs and Martin 1964; Merton 1967; Clinard 1964; Cloward 1959; Cloward and Ohlin 1960; Seeman 1975; for criticisms of this literature, see Clinard 1968; Gibbs and Erickson 1975; Akers 1973).

A central question in this literature has seldom been broached and has never been resolved: Given that so many behaviors are thought to result from anomie, what makes an individual respond to anomie with one type of behavior rather than another? Why do some individuals respond to anomie by committing suicide, while others turn to alcohol and still others join anomie-reducing social movements?[26]

The concepts of suggestion, imitation, and modeling may provide a partial answer to this question. An anomic individual may choose suicide rather than some other solution if a model individual has recently committed suicide—thus bringing suicide to the forefront as a salient solution to anomie.[27] On the other hand, if some alternative solution to suicide were to be made salient, then this alternative might be chosen, instead. Toch (1965, p. 124) provides an excellent example of this when he presents the case history of a woman "switched" from suicide to an alternative solution. "One day [the woman writes] I was sitting in my home and the thought came to me, 'Why don't you put your head in the oven, turn on the gas, and end it all?' Just

[26] Merton attempted a partial answer to this question when he noted that different types of structural conditions might lead to different types of deviant behavior. His approach is incomplete: it helps us to understand why an individual might be prone to retreatist rather than innovative behaviors, but it does not help us to understand why an individual might be prone to one type of retreatist behavior (say suicide) rather than another (say alcoholism). Similarly, his approach does not help us to understand why some individuals respond to anomie with deviance while others respond by joining anomie-reducing social movements. Subsequent modifications of Merton's paradigm (e.g., Cloward 1959; Cloward and Ohlin 1960) can also be shown to provide an incomplete answer to the question under discussion.

[27] Earlier I discussed the imitation of a general mode of problem solving, e.g., using the criteria of decision theory. Here I am concerned with a different type of imitation—the direct imitation of a particular, concrete solution, rather the general imitation of a method of reaching a solution.

then a knock came on the door. I answered, and saw a tall woman. . . . She asked if she could give me Bible studies. I said Yes . . . I was invited to attend some Adventist evangelist meetings. Later I was baptized. For me the Adventist faith is all that I need. It provides an inner peace that passes all understanding."

Here again, as in the analysis of decision theory and of reference group theory, suggestion may serve as a mediating mechanism helping to switch an individual from one choice to another—in this case from a type of deviant behavior to membership in an anomie-reducing religious movement.

I have now considered very briefly some ways one might apply the concepts of imitation, suggestion, and modeling to three areas of social theory. These three theories can be enriched and enlarged if they are extended to incorporate the processes of suggestion, imitation, and modeling. As we become sensitized to processes like suggestion we become aware of very general questions we might otherwise have missed. What switches an individual from one choice to another—from one problem to another, from one mode of solving problems to another, from one reference group to another, from one form of deviance to another? A partial answer to these questions may be found in the concepts of suggestion and imitation, which may serve as very general switching mechanisms in social behavior. If suggestion and imitation do indeed serve these very broad functions, we may find it useful to incorporate the concepts of suggestion and imitation into many theories of social behavior, not just the three theories discussed above.

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Socioeconomic Status and Mental Disorder: New Evidence and a Sociomedical Formulation[1]

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Despite numerous studies which show that socioeconomic status and mental illness are inversely related, it is possible that this relationship exists for some disorders but not others. Study of approximately 10,000 first admissions to state hospitals in one state covering the period 1956–65 shows an inverse relationship only for organic and schizophrenic disorders. In light of the etiological and predisposing factors that are involved in these two disorders, physical and medical factors appear to be more important than psychological (e.g., stress) factors in mediating the relationship with socioeconomic status. It is concluded, therefore, that a sociomedical formulation (in contrast to a sociopsychiatric formulation) is a plausible interpretation of the relationship between socioeconomic status and mental disorder.

Over the years a large number of studies of mental illness have shown that socioeconomic status and mental illness are negatively related (Fried 1969; Dohrenwend and Dohrenwend 1974). A question remains, however, as to whether there are differences by type of disorder. If differences do exist, they would indicate that a specification of the relationship is in order, and this would have implications for the interpretation of the causal role of socioeconomic factors in mental illness. Most interpretations emphasize the mediating role of sociopsychiatric factors, especially socioeconomically induced stress, stemming variously from status frustration, denied opportunity, and unfulfilled aspirations (see Dohrenwend and Dohrenwend 1974, p. 441). In contrast, we will present a causal interpretation in which sociomedical variables are emphasized.

Data are for all 21- to 64-year-old first admissions who were admitted and discharged during the 10-year period between fiscal 1956 and 1965 to each of the six Tennessee state mental hospitals. Diagnostic distinctions are made between brain disorders (due to toxins and organic impairments) and functional disorders, and within the latter, among schizophrenia, manic-depres-

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sion, neuroses, and personality disorders. Of the total of 10,673 patients admitted for mental illness, 9,513 were classified in one of these categories.

FINDINGS

Educational Status

Hospitalization rates are obtained for each of the seven educational categories (0–4, 5–7, 8, 9–11, 12 years, some college, and college graduate and beyond) used to index socioeconomic status. Age-standardized ratios for all patients, separately for males and for females, are calculated for these categories, as follows[2]—males: 119, 110, 113, 103, 85, 77, and 48; females: 111, 108, 122, 107, 89, 61, and 55.

Figure 1 plots the ratios for brain disorders, schizophrenia, and all other disorders (referred to hereafter as "other"). The two highest categories (some college and college graduate and beyond) are combined (with the midpoint set at 14) because of small numbers. For both sexes the inverse relationship is specific to the brain and schizophrenic disorders, with a curvilinear pattern for the other category (manic-depression, neuroses, and personality disorders). Each of the three other disorders exhibits some deviation from the overall trend, although each disorder manifests the same general pattern (data not shown).

Occupational Status

On entering a hospital, patients were classified in one of the occupational categories in the Dictionary of Occupational Titles (DOT) of the Department of Labor. Many of these categories are identical to the detailed occupational categories (DOC) of the U.S. census, and several others are virtually so. Socioeconomic index (SEI) scores are available for the DOC titles (Duncan 1961). The analysis is limited to males.

Since the number of patients (and the population) for some occupations is quite small, a change in just one or two patients could lead to a substantial change in the hospitalization rate. To correct for this (and to follow the

[2] The method for calculating the ratio is the same as that used by the Department of Health, Education, and Welfare in computing mortality ratios (Guralnick 1963*a*, 1963*b*) and is the ratio of tabulated cases in an educational category to the expected number if the rate for each of the several educational categories were the same as for the total sample. The ratio is then multiplied by 100. (The ratio is not to be confused with the rate, which would indicate the number of persons in each educational category who were hospitalized per unit of population.) Rates are based on 8,568 patients from 25 to 64 years old since the census for states does not provide population figures for the 21–24 year olds by education (U.S. Bureau of the Census 1963, pp. 315–16). However, crude rates including 21–24 year olds give the same relationships as those for the age-standardized ratios.

TABLE 1

SOCIOECONOMIC SCORE, HOSPITALIZATION RATES PER 100,000 MALES, AND NUMBER OF MALE ADMISSIONS FOR 23 OCCUPATIONAL CATEGORIES—TENNESSEE, 1956–65

OCCUPATION*	SOCIO-ECONOMIC STATUS SCORE	HOSPITALIZATION RATES PER 100,000			NO. OF ADMISSIONS
		Brain	Schizophrenia	Other	
Teachers (primary, secondary, and n.e.c.)†	78	26	207	194	34
Insurance salesmen‡	76	75	15	313	27
Clergymen	71	205	205	225	32
Electricians	69	246	77	447	50
Stock clerks§	69	116	70	381	21
Salespersons and clerks‖	67	68	92	329	123
Plumbers, gas fitters, and steam fitters#	66	231	71	266	33
Machinists	65	103	88	250	30
Brick and stone masons and tile setters	62	106	42	424	27
Mechanics and repairmen, n.e.c.	62	52	52	165	57
Welders and flame cutters	61	67	134	370	35
Cranemen, derrickmen, hoistmen, and shovelmen**	59	327	109	850	23
Carpenters	58	200	146	305	144
Mechanics and repairmen (motor vehicles)	58	193	171	520	120
Attendants (filling station and parking lots)††	58	144	101	278	48
Painters (construction and maintenance)	56	413	224	508	97
Cooks (except private family)	54	206	103	378	20
Farmers	53	307	216	333	731
Attendants (hospitals and other institutions, n.e.c.)	52	206	124	578	22
Mining and mine machine operators	49	294	52	225	33
Janitors and sextons	47	101	71	131	30
Farm workers	42	129	287	90	143
Porters‡‡	36	159	176	106	30

SOURCE.—For population base (14 years and above) for calculating hospitalization rates, U.S. Bureau of the Census (1963: 411–16).

* Unless otherwise indicated, occupational title is as it appears in both the DOT and DOC classification systems.

† Includes from DOT, "teachers, primary school and kindergarten," "teachers (secondary school) and principals," and "teachers and instructors, n.e.c."; from DOC, "teachers: elementary schools," "teachers: secondary schools," and "teachers, n.e.c.".

‡ Listed as "insurance agents, brokers, and underwriters" in DOC.

§ Listed as "stock clerks and storekeepers" in DOC.

‖ Includes from DOT, "sales clerks," "sales persons," "salesmen, to consumers," and "sales clerks, dry cleaning and laundry"; from DOC, "salesmen and sales clerks, retail trade."

\# Listed as "plumbers and pipe fitters" in DOC.

** Listed as "cranemen, derrickmen, and hoistmen" in DOC.

†† Listed as "attendants, auto service and parking" in DOC.

‡‡ Includes from DOT, "pullman porters," "baggage porters," and "porters, n.e.c."; from DOC, "porters."

procedure used below in the section entitled Evidence for Occupational and Industrial Accidents), all occupations with fewer than 20 admissions are excluded. All residual categories for major occupational and industrial categories (e.g., "professional occupations, not elsewhere classified [n.e.c.]") are also eliminated. This leaves 23 DOT categories which are identical or virtually identical to DOC categories (see table 1).[3] Although the occupations are

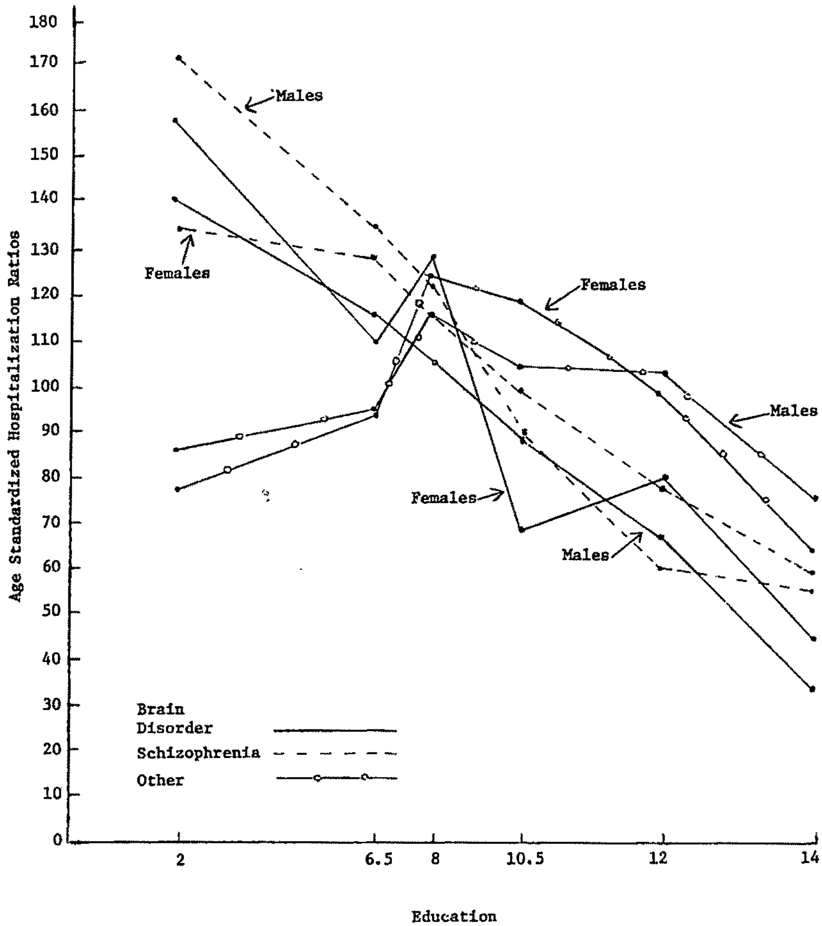

FIG. 1.—Plot of age-standardized hospitalization ratios on midpoint of educational categories—Tennessee, 1956–65.

[3] In seven cases, however, the DOT category has two or three levels (skilled, semiskilled, and unskilled), whereas the DOC category has only one. One category—cranemen, derrickmen, hoistmen, and shovelmen—has three levels, and six categories—auto mechanics and repairmen; mechanics and repairmen (not elsewhere classified); electricians; mining and machine operators; painters, construction and maintenance; and welders and flame cutters—have two levels (skilled and semiskilled). Since most of

clearly not a systematic or random sample (most are manual and service occupations), socioeconomic values from all but the highest levels are represented; and all levels are represented from the blue collar and service categories from 69 for electricians to 36 for porters.

The Spearman rank correlation (ρ) is substantially higher for brain disorders (—.37) and schizophrenia (—.42) than for other disorders (.14). Thus results for occupational status also indicate that the negative relationship of socioeconomic status to mental illness is specific to brain disorders and schizophrenia.[4] Consequently, interpretation of the relationship between

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these occupations require considerable skill, the rule of procedure was to include only those admissions from the highest skill category listed for the occupation in the DOT system. For example, only admissions from the cranemen, et al. category classified as skilled were included in this category, thus excluding all semiskilled and unskilled cranemen, et al.; for welders and flame cutters, only two categories are listed—semiskilled and unskilled, so all the unskilled welders and flame cutters are eliminated. In all cases, however, the rule led to the elimination of very few cases. (Two of 122 for auto mechanics and repairmen; two of 98 for painters, construction and maintenance; one of 51 for electricians; three of 38 for welders and flame cutters; three of 35 for mining and mine machine operators; six of 61 for mechanics and repairmen (not elsewhere classified); and four of 27 for cranemen, derrickmen, hoistmen, and shovelmen.) We believe that the elimination of the semi- and unskilled admissions for occupations with three skill levels is warranted because all of these occupations require considerable skill, even those which are classified in the DOC as in the operative rather than craftsmen category.

[4] Our results are consistent with nearly all the evidence. In his review of the studies of socioeconomic status and schizophrenia, Kohn (1968) reports that almost every study reports a negative relationship (see also Kohn 1976). In their reviews of "true" prevalence studies, the Dohrenwends (1969, 1974) also report that most studies show a negative relationship for schizophrenia but no particular relationship for neurosis and manic-depression. Although sociologists and psychiatrists have generally neglected brain disorders, the limited evidence indicates a negative relationship with class status (Hollingshead and Redlich 1958, p. 233; Jaco 1960, pp. 210–27; see also Langner and Michael 1963, pp. 403, 407). On the surface, findings for personality disorder in this study appear to differ from most studies. The Dohrenwends (1969, p. 30) report that in nine of 12 studies, the maximum rate is reported for the lowest socioeconomic stratum. Comparison of our results and these studies is problematic, however. First, the measurement of social class is imprecise in several of the studies; second, all are studies of prevalence rather than of incidence; and third, all were published between 1931 and 1964. Date of publication is significant since the diagnostic classification system used for patients in this study was developed in 1952 by the American Psychiatric Association (APA) (Committee on Nomenclature and Statistics of the American Psychiatric Association 1952). This classification includes five major categories under personality disorders and 19 specific subcategories which include emotionally unstable personality, passive-aggressive personality, sexual deviation, and alcoholism. Prior to 1952, the diagnosis of personality disorder was often limited to "psychopathic personality" (this is replaced with "sociopathic personality" in the APA classification). In at least four studies cited by the Dohrenwends, this is indeed the case (Hyde and Kingsley 1944; Bremer 1951; Lin 1955; Helgason 1964). In another study, personality disorders are limited to "personality trait" disorders (one of the five major categories for personality disorders in the APA classification scheme), which Langner and Michael admit "lacked clear definition" in the research and included disorders covering a "wide range of . . . mental mechanisms" and etiologies (1963, p. 56). Furthermore, although they do report

socioeconomic status and mental illness must include only those class-related factors involved in the production of these two disorders.

BRAIN DISORDERS

Much of the debate among sociologists, psychiatrists, and others concerns the applicability of the medical model to mental disorders. Sociologists (e.g., Goffman 1961; Scheff 1966, 1974; and Gove 1976, p. 565) contend that it is not valid when applied to mental illness, and some psychiatrists hold the same view (e.g., Szasz 1961; Torrey 1974). The general argument is "that those disorders we call 'mental illnesses' are different in crucial respects from physical illness: they do not have the same biological existence and therefore cannot be understood within the causal framework of natural science [as physical illnesses are understood]. Whereas physical illness is seen as an objective deviation from the norms of physical structure and function, 'mental illnesses' are more properly described as infractions of social norms" (Morgan 1975, p. 268). Such a conclusion, however, would not apply to brain disorders, in which case the medical model is valid (Morgan 1975, p. 265; Torrey 1974, p. 155).

Organic trauma or pathogens are, of course, the precipitating, proximal, or immediate causes of brain disorders. And the pathogens are diverse depending on the specific disease. Consequently, an explanation for the relationship between socioeconomic status and brain disorders must include disorders which have *diverse* physical causes. We suggest there are three general sets of variables: the physical environment, health attitudes and behavior, and medical care.

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a higher rate of these disorders in the lowest socioeconomic stratum (p. 402), there is no tendency for specific symptoms associated with sociopathic personality disorders to be more frequent in the lowest stratum: alcoholic and dyssocial symptomatology were greater in the lowest stratum, but sexual deviance and aggresiveness were three to five times more frequent in the higher strata (p. 407). In still another study (Gillis, Lewis, and Slabbert 1968) figures are given for "symptom patterns" rather than cases, a point the Dohrenwends note (1969, p. 30). Finally, in three studies the differences between the rate for the lowest stratum and the stratum having the minimum rate is small (percentages of .59, .27, and .12) (see Dohrenwend and Dohrenwend 1969, p. 30). Consequently, a change in the socioeconomic classification of very few patients could yield different results. For example, if just one patient were classified from "lower class" to "upper class" in one study, the "upper class" would have the maximum rate (Lin 1953, p. 327). Hence, with very small changes (all of which could easily occur because of measurement or sampling errors), 50% of the studies might report the maximum rate for some stratum other than the lowest. It would seem, therefore, that an inverse relationship between socioeconomic status and personality disorder has not been firmly established. In any case, with the possible exception of personality disorder, findings for the present study are entirely consistent with well-established patterns in research on socioeconomic status and mental disorder. They are also consistent with evidence for most differences by sex (see the review by Gove and Tudor 1973; also Dohrenwend and Dohrenwend 1977) and marital status (Gove 1972). Differences by sex and marital status are topics of forthcoming papers.

Physical Environment

We include here hazards that are apt to contribute to accidents (e.g., occupational hazards) as well as the more general cultural and social components of the environment (e.g., human aggression). Evidence shows that accidents and injuries to adults are more frequent in the lower socioeconomic strata (Sanders 1964, p. 670). Also, low-status occupations are disproportionately manual and hence more physically hazardous than high-status jobs. Although some authors question whether aggressive behavior is more frequent in the lower socioeconomic strata (see Straus 1971; Steinmetz 1974), others believe that aggression is negatively related to socioeconomic status (see Miller 1958; Goode 1971; and Banfield 1974, pp. 61–64). Most evidence seems to support the latter position. Lower-status persons (classes 4 and 5 on Hollingshead's Two-Factor Index) are more apt to file complaints of physical abuse in divorce proceedings (Levinger 1966); lower status parents are more apt to use physical punishment (see Gold 1958; Bronfenbrenner 1958; and Kohn 1969); and children in lower socioeconomic, poverty, and welfare families are more apt to be victims of child abuse (see Gil 1970; and Erlanger 1974). In addition, anecdotal and ethnographic accounts would indicate support for this belief (see Winslow 1968, pp. 122–23).[5]

Although physical accidents and violence only rarely result in brain damage, an increase in their frequency increases the probability of damage to the body including the brain. Consequently, the fact that physical accidents and violence are more frequent in the lower socioeconomic strata increases the probability of injuries to the brain.[6]

Health Attitudes and Behavior

A review of studies of health-relevant attitudes and behavior indicates that those typical of both lower-strata persons and health-care providers cause

[5] The following comments from teenage slum dwellers illustrate this (Winslow 1968, pp. 122–23). "When I first started living around here it was really bad, but I have gotten used to it now. Been here two years. People getting beat up. . . . Gee, there's a lot of violence around here. You see it all the time. . . ." "Sometimes where I live at people be hitting at each other, fighting next door. Then when they stop fighting then you can get some sleep. . . ." "Drinking, cussing, stabbing people, have policemen running all around mostly every day in the summertime." (See also Toby 1966.) Two studies which raise questions concerning the negative relationship between socioeconomic status and violence are attitude and value studies of college students (Straus 1971; Steinmetz 1974); as a result the socioeconomic scale is necessarily truncated at the lower end.

[6] Note that this formulation stipulates that mental illness is an outgrowth of violence rather than the reverse. Others who observe that violence and mental illness are more frequent in the lower socioeconomic strata view mental illness as a cause of violence. Banfield, for example, states that while "lower class life [is] extraordinarily violent, . . . much of the violence is probably more an expression of mental illness than of class culture" (Banfield 1974, p. 63). Our position is that the higher rate of mental illness in the lower class is due, in part, to a higher rate of violence.

lower-status persons to use health services less than is medically indicated (Berkanovic and Reeder 1974). Such persons know less about the treatability and severity of symptoms, have less confidence in the efficacy of medical treatment, and make less use of preventive and other discretionary services (Ross 1962; Richardson 1972; Rosenstock 1969; Bice, Eichhorn, and Fox 1972). Poor personal hygiene and failure to take necessary preventive measures to avoid contracting a disease (e.g., syphilis) may be related to class status. Also, excessive drinking is probably not as severely sanctioned in the lower strata and the dangers of prolonged drinking may not be as well known. Thus, attitudes and behavior as aspects of cultural and social life-style may be contributing factors to the higher rates of brain disorder in the lower socioeconomic strata.

Medical Care

The bulk of the evidence on physical illness indicates that the rates of mortality and morbidity are higher in the lower socioeconomic strata (Antonovsky 1972; Mechanic 1968, pp. 236–61; Richardson 1972). Since members of the lower strata apparently do not now receive a disproportionately lower proportion of health care than higher socioeconomic groups (see reviews by Bice et al. 1972; Anderson and Anderson 1977), the higher illness rates in the lower strata may be due to other factors such as public sanitation, personal hygiene, and health practices and attitudes. Some evidence suggests, however, that once need for medical care is controlled, physician contacts may still be positively related to socioeconomic status (Miles 1977). More significantly, even though socioeconomic differences in medical care may be small today, substantial differences existed before the patients in this study were hospitalized (1956–65) (Bice et al. 1972; Anderson and Anderson 1977). Moreover, lower-status persons are more apt to receive care in hospital clinics and emergency rooms and to be set apart when they are hospitalized; they are less apt to have regular physicians and more apt to be seen by chiropractors, foreign-educated physicians, or physicians' assistants (Department of Health, Education, and Welfare 1972; Herman 1972; Roth 1969; Duff and Hollingshead 1968; Alpert et al. 1969; Studnicki, Saywell, and Wiecheter 1976; Koos 1954; Rushing and Miles 1977). These differences in types of care, and differences in preventive care may contribute to failures to detect certain conditions that may subsequently result in brain damage: epilepsy, Huntington's chorea, and pernicious anemia, all of which may be associated with schizophrenia (see Davidson and Bagley 1969; and Brune 1971) as well as syphilis, brain tumors, intracranial lesions, and possibly alcoholism. Also, the lack of a regular physician and hence continuity of care may contribute to intrauterine infection, congenital defects, birth

trauma, and other conditions that result in brain disorders; and evidence does show that both infant mortality and postneonatal mortality rates are higher in lower-status groups (Lerner 1969, pp. 94–100).

In summary, the inverse relationship between socioeconomic status and brain disorders may be due in large part to such things as the physical environment, health attitudes and behavior, and access to quality medical care. Evidence for occupations and industry is consistent with the formulation as it pertains to the physical environment.

EVIDENCE FOR OCCUPATIONAL AND INDUSTRIAL ACCIDENTS

If socioeconomic factors which contribute to bodily injury in general contribute also to brain injury, occupations with higher rates of accidental injuries should also have higher rates of brain disorders in comparison with rates of functional disorders. Unfortunately, data are not available for accident rates by occupation. However, Guralnick (1963*a*, 1963*b*) reports 1950 male mortality rates for occupational categories included in the U.S. Census Intermediate Occupational Classification (IOC). The age-standardized mortality ratio (SMR) is reported for each category if there were at least 20 deaths. There are 13 categories that are the same or virtually the same as those in the list of 23 from the DOT system in table 1. These occupations (and 6 others) are presented in table 2.

For the 13 occupations, ρ between SMR and brain rate is .74 in comparison with .05 and .29 for the schizophrenic and other rates (for all functional disorders combined, $\rho = .29$). Since results could be affected by the small number of occupations, all occupations are included with reported SMRs if they had at least 10 hospital admissions, giving six additional cases. (As it turns out, all of these occupations have 15 or more admissions.) The ρ for the brain rate is .44 in comparison with .05 and .01 for the schizophrenic and other rates (.09 for all functional disorders). Results thus support the view that factors associated with injury to the body are contributing factors to impairment of the brain.

The DOT classifies a number of blue-collar manual occupations according to industries, seven of which are identical or approximately so to industries for which SMRs are reported by Guralnick (1963*b*) (see table 3). Schizophrenics and patients in the other category are combined because of the small number of cases for certain industry categories. Also, since state population is not given in the census for all categories in table 3, the ratio of brain to functional disorders is given rather than rates for the industrial population. Although there are few cases, only two of which permit exact comparisons, and most white-collar occupations are excluded, the correspondence in the ranking for the SMR and the ratio is clear; $\rho = .57$.

Results may be distorted because individual industries contain occupations that are heterogeneous in respect to physical danger and job-related accidental deaths. In one instance, however, data are available for the same occupation for different industries. Both the IOC and DOT classifications distinguish between "painters, construction and maintenance" and "painters, except construction." Since the SMR for the construction industry is relatively high (181), we would expect a higher brain to functional disorder ratio for painters in this industry than for other painters. Results show this to be the case. The ratio for nonconstruction painters is 0.167 (1:6) in comparison with 0.508 (32:63) for painters in the construction industry. Comparisons are based on painters classified in the skilled category in the DOT

TABLE 2

AGE-STANDARDIZED JOB-RELATED ACCIDENTAL MORTALITY RATIOS FOR ALL U.S. MALES, 20–64, 1950, AND HOSPITALIZATION RATES FOR TENNESSEE, 1956–65

Occupation*	Job-related Accidental Mortality Ratio (SMR)	Hospitalization Rates per 100,000			
		Brain	Schizophrenia	Other	No.
Lumbermen, raftsmen, and woodchoppers	844	151	176	187	18
Mine operatives and laborers (n.e.c.)	581	294	225	52	36
Linemen and servicemen (telegraph, telephone, and power)	429	67	175	88	15
Cranemen, hoistmen, and construction machinery operators†	273	329	850	109	30
Electricians	231	246	447	77	52
Plumbers and pipe fitters‡	136	248	266	71	34
Painters (construction and maintenance)	131	413	508	224	102
Policemen, sheriffs, and marshalls§	130	168	223	28	17
Farmers and farm laborers	114	254	274	233	999
Welders and flame-cutters	107	67	370	134	40
Guards and watchmen‖	106	175	153	102	19
Carpenters	93	205	305	146	148
Civil engineers	88	111	295	148	15
Tinsmiths, coppersmiths, and sheet metal workers	85	202	404	202	16
Other mechanics and repairmen and loom fixers	77	56	165	51	65
Masons, tile setters, and stone cutters#	69	106	424	42	29
Mechanics and repairmen (automobile)	64	200	520	171	129
Janitors and porters**	49	101	131	71	61
Machinists	46	102	250	88	31

SOURCES.—Guralnick (1963*b*) for SMRs, and U.S. Bureau of the Census (1963) for population base (14 years and above) in calculating hospitalization rates.

* Occupational title is as listed in the IOC.

† Listed as "cranemen, derrickmen, hoistmen, and shovelmen" in DOT.

‡ Listed as "plumbers, gas fitters, and steam fitters" in DOT.

§ Includes "policemen and detectives (public and private)" and "sheriffs and bailiffs" from DOT.

‖ Listed as "guards and watchmen, except crossing watchmen" in DOT.

\# Listed as "brickmasons, stonemasons, and tile setters" in DOT.

** Includes "janitors and sextons," "pullman porters," "baggage porters," "porters, n.e.c." from DOT.

TABLE 3

STANDARDIZED MORTALITY RATIO FOR JOB-RELATED ACCIDENTAL MORTALITY FOR 20- TO 64-YEAR-OLD MALES, UNITED STATES, 1950, AND RATIO OF BRAIN TO FUNCTIONAL DISORDER ADMISSIONS TO STATE MENTAL HOSPITALS IN TENNESSEE, 1956-65, BY INDUSTRY

Industry*	Age Standardized Mortality Ratio (SMR)	Brain to Functional Disorder Ratio	No. of Hospital Admissions
Logging†	1,031	.50	21
Mining‡	435	.81	38
Transportation, communication, and other public utilities	190	.34	473
Construction	181	.63	353
Stone and clay products§	133	.55	14
Paper and allied products‖	79	.00	6
Printing, publishing, and allied industries#	28	.22	22

SOURCE.—For SMRs, Guralnick (1963*b*).
* Industry is listed as in Guralnick (1963*b*).
† Occupations included from the DOT classification are "inspectors, scalers, and graders, log and lumber" and "lumbermen, raftsmen, and woodchoppers."
‡ Occupations from the DOT classification are those listed under "occupations in extraction of minerals."
§ Occupations from the DOT classification are those listed under "occupations in production of stone, clay, and glass products."
‖ Occupations from the DOT classification are those listed under "occupations in production of paper and paper goods."
\# Occupations from the DOT classification are those listed under "printing occupations."

system. If semiskilled and unskilled are included (see n. 2), the comparisons are 0.09 (1:11) and 0.515 (33:64).[7]

Thus, as stated in our conception of physical environment, socioeconomic factors which contribute to physical illness and injury in general appear also to be involved in the etiology of toxic-organic mental disorders.

SCHIZOPHRENIA

Several biological and physiological factors are involved in schizophrenia that may be related to socioeconomic status. They are classified as genetics, biochemical and neurological factors, and viruses.

Genetics

A number of studies, twin studies in particular, provide compelling evidence that heredity is an important factor in schizophrenia (see Shields 1968;

[7] The category "painters, except construction and maintenance" is not included in table 1 because it did not have at least 20 admissions in the skilled level. (Inclusion of all skill levels is appropriate when comparisons are among industries; their inclusion is questionable when comparisons are for occupations of different socioeconomic status.) Since the socioeconomic scores of the two painter categories are virtually the same (56 and 57), the industry comparisons would indicate that physical hazards associated with work are more crucial in the brain disorders than socioeconomic status per se.

Gottesman and Shields 1972; Campion and Tucker 1973; Kety 1974; Boklage 1977). As an explanation, some believe brain lateralization is important, with "the primary focus of schizophrenia brain dysfunction [being] found in the left hemisphere" (Boklage 1977, p. 32). Others observe that the hereditary factors which contribute to certain organic conditions may also be involved in schizophrenia (Taylor 1977; and Trimbel 1977). Significantly, behavior associated with hereditary schizophrenia is similar to that found in organic schizophrenia and in brain disorders with "schizophrenia-like" symptoms, such as epilepsy, especially temporal lobe epilepsy (Slater, Beard, and Glithero 1963; Taylor 1977, p. 404). But studies have yet to reveal precisely what the genetic factors are (e.g., see Shields 1968, pp. 111–17).

It is even less clear how genetic factors might account for the role of socioeconomic status in schizophrenia. One possibility, of course, is that (hereditary) schizophrenics have greater difficulty performing high-status occupational roles. Consequently, they "drift" downward (or remain at their beginning points) in the class structure. On balance, however, a review of the evidence would indicate that drift is not the sole explanation (Kohn 1968, pp. 158–60). We will have more to say about this hypothesis later.

For the present we will simply note that class differences in medical care provide an explanation that is as plausible as the drift hypothesis. Note first that it is not schizophrenia itself that is inherited. "Schizophrenic psychosis is clearly not what is inherited; that takes on the average near 30 years to appear, and it is not possible in the majority of cases to say that the individual 'was always more or less like that.' Therefore, what is inherited or otherwise cellularly imposed before birth is some cellular *anlage*, stable to 20–60 years of growth with or without intervening development of the *anlage* itself or of identifiable behavioral deviations originating therefrom" (Boklage 1977, p. 30). Taylor believes that "schizophrenic symptomatology results from disorganization of the structures subserving language and thought (and hence also socialization), normally compactly located in the left brain. *Developmental* anomalies which lead to alternative strategies in locating these skills lead to diffusion of the necessary association networks. In the *course of development* the neural base of language contracts first from hemispheric equipotentiality to the left hemisphere, and *later* contracts within the hemisphere to very localized areas" (1977, p. 406; emphasis added). In this view the onset of hereditary schizophrenia would vary with age and is precipitated by *developmental* anomaly: "Many different types of developmental anomaly may therefore lead to the deployment of a thinking and behavioral strategy which we call schizophrenia" (ibid.). Schizophrenia may also depend on damage incurred during brain development (ibid.).

If schizophrenia is not itself inherited but develops out of inherited pre-

dispositions, many cases might be prevented if proper medical care were provided. Indeed, Taylor notes that alert pediatricians may be able to identify symptoms which are precursors of schizophrenia later in life (1977, p. 406). Further, the risk of schizophrenia is disproportionately high for persons with temporal lobe epilepsy (Taylor 1975), and both schizophrenic symptoms and epileptic seizures are known to be influenced by the same drug, dopamine (Trimble 1977).

All this suggests that hereditary vulnerabilities or predispositions toward schizophrenia may be prevented by the intervention of good specialized medical care, which may be inversely related to class status. Therefore, the fact that genetic factors may be paramount in schizophrenia does not necessarily make environmental factors, such as socioeconomic status, any less important (for comments on the environment in general, see Shields 1968).[8]

Biochemical and Neurological Factors

Symthies (1975) and Trimble (1977) theorize that existing evidence indicates that biochemical factors are involved in schizophrenia. The evidence for neurological factors is substantial, with a number of conditions (e.g., nonlocalized neurological damage, irregular motor responses and muscle tone, EEG abnormalities, and central nervous system dysfunctions) found to be associated with schizophrenia (for a review see Campion and Tucker 1973, p. 461). Taylor considers the evidence "quite formidable" and so compelling that he wonders about the validity of the whole conception of functional psychoses (1977, pp. 405–6). It is not clear, however, whether the neurological deficits are due to heredity or to prenatal factors (Campion and Tucker 1973, p. 461). It is also not clear how they would be related to socioeconomic status. For example, schizophrenics and nonschizophrenics differ in the functioning of the extensor digitorum brevis muscle (Goode et al. 1977), but there is no reason to believe that the functioning of this

[8] Research indicates that genetic factors are also important in manic-depression (see Allen 1976 and Gershon, Dunner, and Goodwin 1971). We have seen, however, that manic-depression is not significantly related to educational status. Although this appears to be consistent with reviews of research on socioeconomic status and manic-depression (see n. 4 above), the lack of a relationship may be due to the relatively small number of cases of manic-depressives in the present study (170 males and 308 females). Still another possibility is that different indices of socioeconomic status produce different results depending on the disorder; this issue is explored in a forthcoming paper. It is also conceivable that the genetic component in manic-depression does not interact with medical care, or at least with the class determinants of access to medical care, as we have suggested for schizophrenia. Significantly, in their review of the evidence concerning a genetic component in the affective psychoses, Gershon et al. (1971) do not even mention the possibility of interaction between socioeconomic factors and genetic components. However they do discuss the interaction of parental loss and parental deprivation with genetic factors as possibly significant in the etiology of depressive psychoses (1971, esp. pp. 5–6).

muscle varies by socioeconomic status. In other instances, however, linkages are indicated.

Evidence shows that individuals with brain disorders may exhibit symptoms similar to those of schizophrenia (Brackbill and Fine 1956; Davidson and Bagley 1969; Torrey and Peterson 1976). These disorders include Huntington's chorea, general paresis, pernicious anemia, and disorders with an epileptic base (for reviews see Davidson and Bagley 1969; Brune 1971). Evidence also shows that patients with brain tumors and persons who experience cerebral trauma frequently manifest schizophrenia-like symptoms, as do persons with lupus (Remington and Rubert 1962; Slater et al. 1963; Davidson and Bagley 1969; Klerman 1975, p. 86; Taylor 1975), and, as noted previously, epilepsy and schizophrenia appear to be related to a common biochemical mechanism since each responds to dopamine (Trimble 1977). Many of these factors could contribute to the inverse relationship between socioeconomic status and schizophrenia through two routes.

First, the effects of most, if not all, of the neurological impairments might be minimized with appropriate medical treatment, and access to high quality medical care and utilization of preventive care are positively related to socioeconomic status (see above). Even those who question whether the genetic interpretations are not overstated indicate the possible role of socioeconomic factors in neurological deficits. Specifically, Campion and Tucker (1973) believe that some of the results of twin studies could be accounted for by such factors as intrauterine insufficiency, perinatal hypoxia, and birth trauma. These conditions contribute to prenatal damage and infant mortality. The latter is known to be inversely related to socioeconomic status (Lerner 1969) and the former probably is. Consequently, Campion and Tucker (1973, p. 463) properly caution that information is needed about the class background of subjects in the twin studies. In some instances, of course, it may be questioned whether the schizophrenia-like thought and behavior really constitute schizophrenia (for a discussion, see Davidson and Bagley 1969, pp. 151–53). This would clearly be the case when behavior due to brain tumors and general paresis is diagnosed as due to schizophrenia. In these instances it would probably be correct to speak of the *mis*diagnosis of schizophrenia.[9] And this is the second socioeconomic route through which neurological impairments may operate. We have already seen that brain disorders are inversely related to socioeconomic status. Although the reliability of brain diagnoses would appear to be relatively high (see Torrey 1974, p. 67), errors do occur. Consequently, erroneous schizophrenic diag-

[9] The issue is further confused because schizophrenia is probably not a unitary disease with a common etiology but "is comprised of various disorders" with "multiple determinants in which there is not certainty as to the exact etiology" (Klerman 1975, p. 84). Nevertheless, some evidence would suggest that agreement among psychiatrists as to which patients are schizophrenic is relatively high (Kety 1974).

noses *typically* associated with brain disorders would also be related to socioeconomic status.

Viruses

A review of clinical, epidemiological, and laboratory evidence indicates that viruses are involved in the etiology of schizophrenia (Torrey and Peterson 1976). Although an understanding of this possible involvement is particularly difficult because of immunizations and the existence of "slow viruses" (viruses that remain latent and asymptomatic for periods of five to 20 years), evidence suggests that viral encephalitides, in particular, may be involved in schizophrenia (ibid.), and this has been noted for a number of years (Hendrick 1928, p. 1009). In addition, studies indicate unusually higher incidence of abnormal dermatoglyphics (fingerprints, palm prints, and footprints) and abnormal capillary formations in schizophrenics (Torrey and Peterson 1976, p. 139). Both conditions may result from intrauterine viral infections. It has also been reported that several studies in Sweden, Norway, Denmark, Great Britain, and the United States show a disproportionate number of schizophrenics being born in January through April, a period in which many viruses (e.g., rubella, measles, varicella-zonter) show a similar peak of incidence (ibid., p. 140).

The vital hypothesis of schizophrenia is consistent with the findings for socioeconomic status on two counts. First, individuals can be immunized against many viral diseases and otherwise protected through preventive measures. While mass immunizations have certainly reduced socioeconomic differences, individuals in the lower strata are probably less apt to be immunized especially if it depends on purposive action since lower-class persons are less apt to be knowledgeable about the need for immunization. They are also less apt to recognize the value of preventive practices in general or to have a continuous relationship with a personal physician who would urge that preventive steps were taken. And lower-class mothers may receive lower quality prenatal care, thus increasing risks of intrauterine viral infections which affect the fetus.

Second, people must live in close and regular contact before viruses can be maintained in the community (Torrey and Peterson 1976, p. 140). Some viral diseases became prevalent only with the development of urbanization. Thus they may be most prevalent in the lower strata because of crowded living conditions, particularly in urban areas.

Summary

Socioeconomic factors may perform significant etiologic roles in brain disorders and schizophrenia in two ways. First, lower-status individuals may

be more apt to be exposed to etiological factors (e.g., infectious diseases) of both disorders. Second, socioeconomic factors may mediate the effect of biological factors (e.g., infectious diseases, neurological deficits). Many of the factors associated with brain and schizophrenic disorders may be modified by appropriate health-relevant attitudes and behavior (e.g., preventive care) and medical attention, which may vary by socioeconomic status. Therefore, rather than the relationship of a sociological variable with mental illness calling for the rejection of the medical model as is commonly supposed by sociologists, the integration of medical and sociological factors in a common (sociomedical) interpretation is called for instead.

ALTERNATIVE FORMULATIONS

Other interpretations of the relationship between socioeconomic status and mental disorder may be classified into three categories: drift or social selection, socioeconomic stress, and labeling.

Drift

For both brain disorders and schizophrenia, drift is controlled to some extent with the measure of education; psychiatric status probably has less effect on education than on occupation or income since the education of most persons is completed prior to adulthood. Still it might be argued that in some cases of brain disorder (e.g., such as congenital disorders and disorders due to birth trauma), the condition causes low educational achievement. This might be at least partially responsible for the high rate of brain disorders for patients with little or no education. However, there are only 26 cases with congenital and birth trauma diagnoses in the sample; their removal has no effect on the relationship in figure 1. Also, it is not clear why persons with childhood and adolescence disorders would not be hospitalized until after they were over 20 years of age. It is conceivable, of course, that many individuals are cared for by parents until adulthood, at which point they are placed in institutions. It this were the explanation for our results, unusually high rates would be expected for young adults with low educations. Table 4 shows, however, that this is not the case. With only a few exceptions, brain disorders and schizophrenia tend to decrease as education increases for all age categories.

In addition, the nature of the relationship would appear not to be consistent with a drift formulation. Figure 1 shows that for males, brain disorder and schizophrenic rates continuously decrease with educational status and, with only two exceptions for brain disorders, the same is true for females. Also, occupational findings for males indicate a linear trend for each disorder. Furthermore, the scatter diagram for occupational status reveals that,

with the exception of a few deviant cases, the relationship is gradual with a generally consistent decline in rates of brain disorder and schizophrenia as occupational status increases. It is not likely that the effect of mental illness on class status would have such a constant effect at all levels of the class structure.[10]

Note that occupations in the middle and upper range of socioeconomic values require more than simple occupational skills; it seems most doubtful that differences between occupations in this range would be due to social

TABLE 4

CRUDE HOSPITALIZATION RATES PER 100,000 POPULATION BY AGE, EDUCATION, AND DIAGNOSIS—25- TO 64-YEAR-OLD MALES AND FEMALES, TENNESSEE, 1956–65

	Education					
Age and Diagnosis	0–4	5–7	8	9–11	12	+12
25–29:						
Brain disorders...	146	87	66	45	30	11
Schizophrenia....	428	398	285	295	173	134
Other...........	173	269	266	392	275	152
30–34:						
Brain disorders...	166	100	92	55	20	14
Schizophrenia....	326	390	361	277	204	150
Other...........	171	276	341	341	236	160
35–39:						
Brain disorders...	211	131	88	57	37	23
Schizophrenia....	258	247	225	250	151	128
Other...........	249	274	290	276	238	177
40–44:						
Brain disorders...	189	100	72	66	51	51
Schizophrenia....	343	215	223	120	125	85
Other...........	250	295	315	284	284	255
45–49:						
Brain disorders...	160	112	91	118	97	32
Schizophrenia....	182	147	114	107	99	85
Other...........	280	235	401	352	348	207
50–54:						
Brain disorders...	250	171	147	91	122	58
Schizophrenia....	86	119	118	55	67	42
Other...........	263	270	364	375	347	203
55–59:						
Brain disorders...	263	234	284	192	172	85
Schizophrenia....	63	44	64	57	80	30
Other...........	206	232	295	279	322	231
60–64:						
Brain disorders...	429	330	439	228	417	194
Schizophrenia....	41	21	29	23	43	28
Other...........	95	144	215	154	238	175

SOURCE.—Population base: U.S. Bureau of the Census (1963: 315–16).

[10] According to Dunham (1965, p. 191), a drift effect for schizophrenia would probably be most apparent if there were an unusually high schizophrenic rate for the lowest stratum, with little or no difference between positions in the middle and upper range of socioeconomic values.

selection based on "mental health." For example, although teachers have a higher socioeconomic score than electricians, the skills of the latter are sufficiently high so that persons who are incapable of performing as electricians for psychiatric reasons are not likely to be capable of functioning as teachers. Consequently, the difference in rates of brain disorder (39 vs. 231) is probably not due solely to drift. (Other comparisons may be made from table 1 for both brain disorders and schizophrenia [e.g., rate of 68 for salespersons and clerks vs. rate of 390 for construction and maintenance painters].) It would seem, then, that without rejecting the role of drift altogether, there is something about socioeconomic status that contributes to schizophrenia (Kohn 1976) and to mental disorders with an organic base.

Socioeconomic Stress

Most interpretations of the role of socioeconomic factors in mental illness emphasize greater stress in the lower socioeconomic strata stemming from such things as blocked aspirations and status frustration. Obviously such frameworks are not valid for brain disorders since these disorders have a physical basis and are not due to socially induced psychological stress.

They also do not provide very plausible accounts for schizophrenia. The most coherent and explicit conceptual framework which emphasizes stress is Kohn's formulation (1972*a*, 1972*b*, 1976), which states that genetic vulnerability together with stress and limited-rigid conceptions of reality (both of which Kohn believes are more prevalent among lower-class individuals), combine to produce a high rate of schizophrenia in the lower class. The formulation seems to assume that genetic vulnerability, stress, and the impaired ability to deal with problematic and stressful situations are all higher for members of the lower socioeconomic strata"[11] (Kohn 1976, p. 180). Unfortunately for this framework, there is little evidence that lower-status individuals suffer from more situationally induced stress or that they are not as well equipped to deal with it when stressful events occur.[12] Also, while

[11] Actually, the formulation need not necessarily stipulate that all three variables are higher in the lower socioeconomic strata. There may be statistical interactions among the three factors and class status so that forces generating schizophrenia may be greater in the lower strata even though the values of each contributing factor are not.

[12] The literature on stress, especially as related to socioeconomic status, is ambiguous at best. This is partly due to the definition and measurement of stress and the designation of external events which are stress inducing. For example, the Dohrenwends view stress as the consequence of "stressors," defined as "objective events that disrupt or threaten to disrupt the individual's usual activities" (1973, p. 115). One may wonder whether disruption of usual activities produces stress any more frequently than the absence of disruption. In any case, the Dohrenwends conclude that "there is no firm evidence that the overall rate of stressors varies with social class" (ibid., p. 119). At the same time, they apparently believe that lower-class persons are less able to cope with stress, either because of external factors (e.g., support from family) or internal

there is some limited evidence to suggest that life changes and problems in living are precipitating factors in schizophrenia (Rogler and Hollingshead 1965; Brown and Birley 1968; Birley and Brown 1970), the evidence for stress itself is far from compelling (Mechanic 1972). One review indicates that the evidence is too unsatisfactory to permit definitive conclusions about a relationship between stress and mental illness (Jaco 1973). More significantly, there appears to be no evidence showing that stress associated with schizophrenia is linked to socioeconomic status.[13] In comparison, there is substantial evidence that a number of biological and physiological factors, which would appear to be related to socioeconomic status, are most definitely involved in schizophrenic disorders.[14]

Labeling

It might be argued that the present findings are due to differential societal responses to individuals of different socioeconomic status who do not really differ in mental status. Some evidence does indicate that responses of mental health professionals may be influenced by the socioeconomic status of patients (Gardner and Badigan 1966; Fried 1969, p. 115; Michael 1967). It would seem, however, that this would make for a stronger relationship for

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factors (e.g., lower intelligence). Hence, although they recognize serious limitations on the quality of the data, they suggest that the best hypothesis is that severity of stress is greater in the lower class than in the middle class (ibid., p. 132). We are not convinced. This is due partly to the definition of stressor, partly to the authors' assumptions as to how various factors (e.g., intelligence) mediate stress, and partly to the questionable effects (which the Dohrenwends recognize) that certain attitudinal factors have in mediating the effects of stressors. Finally, there is no actual evidence that stress is higher in one class than in another. We are also inclined to believe that the adaptive and coping ability of the socioeconomically disadvantaged is far greater than many suppose it to be. People learn to cope by developing skills to handle difficult life situations, and, as David Mechanic notes, the disadvantaged have more opportunity to develop such skills than persons more fortunate do (1972, p. 307). Glen Elder's (1974) study of the children of the depression is particularly relevant to this point. We believe that the only definitive conclusion one can make about socioeconomic status and stress is that the relationship is either nonexistent or the evidence is too limited and ambiguous to permit a conclusion.

[13] For example, although Kohn (1976) cites Rogler and Hollingshead's study (1965) as supporting evidence for his stress formulation, this study produces no evidence showing that rates of schizophrenia are higher in the lower class because of stress. This study does not give rates by class, since it was limited to lower-class subjects—a sample of lower-class schizophrenics and a sample of matched controls. Our point is not that Kohn fails to recognize this, since he does (e.g., 1972*a*, p. 298). Rather, it is to note that even if psychic stress is related to schizophrenia there is no evidence that such stress is linked to socioeconomic status (on this point, see especially Brown and Birley 1968, p. 212).

[14] This evidence would not be inconsistent, of course, with physiological and biochemical models of stress (e.g., Selye 1956). To the extent that mental illness has a biological base, it could be a consequence of stress on the physiological and biochemical systems.

the other disorders. There is probably less class bias, or error from any source, in organic than in functional diagnoses overall,[15] and evidence would indicate that interjudge reliability of schizophrenia diagnosis is reasonably high (Kety 1974, p. 960).

IMPLICATIONS

One implication of our analysis is that the results of studies which have reported a relationship between socioeconomic status and mental illness may be primarily due to the inclusion of patients with brain and schizophrenic disorders, or their precursors, in the analysis. If our interpretation is valid, the overall relationship is due more to the role of medical rather than psychiatric and psychological factors. A relationship of socioeconomic status to mental illness is thus consistent with the medical model.

At the same time, the explanation we offer is basically sociological. The various factors which have been postulated as significant are aspects of social locations in society that differentiate between groups and categories of persons. In a very general sense they may be viewed as components of class "subcultures," referring as they do to a range of environmental conditions, behavior patterns and attitudes, and access to medical care which distinguish among socioeconomic strata in society.[16] The interpretation thus emphasizes the role of socioeconomic factors in creating mental illness either through direct action on physical states or in mediating the effects of physical conditions which are conducive to the production of mental illness. The formulation would appear to be specific to brain and schizophrenic disorders since data do not indicate that other disorders (manic-depression, neuroses, and personality disorders) are associated with socioeconomic status (but

15 Torrey states that "the signs and symptoms of possible brain disease can be taught to a nondoctor in one or two weeks . . ." (1974, p. 67).

16 It is also conceivable that findings for both brain and schizophrenic disorders reflect a tendency for higher status persons to seek care in the private sector. This, however, would not explain why such a tendency does not exist among persons in the "other" category. Also, in most instances private psychiatric care requires substantial economic resources which are associated with and largely determined by educational achievement as well as verbal and intellectual capacities which also come largely as a result of education. It is likely that the most significant distinction between individuals in both these respects is between the college educated and the noncollege educated. Results indicate, however, that these are not the places where significant breaks occur in the relationships for brain disorders and schizophrenia (see figure 1); overall, the relationships are essentially linear. In addition, a negative relationship with socioeconomic status has been reported in studies even when private hospital patients have been included (e.g., Hollingshead and Redlich 1958; Dunham 1965). Also, Dunham reports that almost all patients of private practitioners spent time as hospital inpatients (1965, p. 262); and Pasamanick, Scarpiti, and Dinitz (1967, p. 138) find that the socioeconomic distribution for state hospital schizophrenics is similar to that for schizophrenics referred to a treatment center by private physicians and welfare and other agencies.

see n. 4 above), and there is less reason to believe that biological factors are important in most of these other disorders (but see n. 8 above).

There is a policy implication here. To the extent that differences in brain disorders and schizophrenia are due to differences in access to quality medical care, government policy which reduces class differences in medical care would reduce class differences in mental illness. This would be the case even if differences in access to mental health professionals were unchanged. According to our formulation, factors which are involved in the class-related etiology of mental illness have an effect on mental states because they first have either a direct or a mediating effect on physical states. Hence, programs which remove economic barriers to quality (nonpsychiatric) medical care should reduce class differences in mental illness as well as in mortality and other forms of morbidity. Also, the elimination of occupational hazards would probably reduce the rate of brain disorders for low-status occupations.[17]

Finally, in concluding, we would make three points.

First, our focus on an integrated sociomedical model interpretation does not altogether deny the validity of various views which contend that the medical model is inapplicable for understanding mental disorders. Our findings are for only one variable among many. "Problems in living," labeling, conflicting expectations, crowded and urban living, even stress—all may be factors in mental illness, and, indeed, they may be more important than socioeconomic status. Our effort has not been to provide an overall theory of mental illness/hospitalization, or even of brain and schizophrenic disorders.

Second, our formulation stipulates that the relevant socioeconomic factors are numerous. There is no *one* socioeconomic factor that is primary. There are many specific factors,[18] most of which can be classified in the categories of physical environment, attitudes and behavior, medical care, and exposure to infectious agents. Therefore, by "integrated sociomedical model" we mean that various aspects of the social environment have direct or mediating effects on biological conditions which in turn have physiological and organic

[17] We note, however, that a policy implication is not the same as a policy recommendation. Whether the above policies are to be recommended would depend on how much they would cost in comparison with the benefits they would generate. This is difficult to assess because, while the costs of such programs can be estimated with a reasonable degree of accuracy (at least in economic terms), the benefits cannot (e.g., the number of cases of mental illness that would be prevented is almost impossible to estimate).

[18] In a similar vein, Fried states: "It may be . . . that our usual expectation that a single major factor will account for a large part of the variance is inappropriate for the investigation of psychiatric disorder, that many diverse factors have minor and independent determining effects and that only their cumulative effect serves as a potent and highly general determinant of mental illness" (1969, p. 163). In contrast, however, Fried's focus, as most others, is on intervening psychological states (1969, pp. 137–55, especially at 155—"one can only conclude that if poverty per se is closely linked to psychiatric disorder, it must be through some psychological mechanism . . .").

consequences that can be understood only in terms of a medical model.

Third, although our results are consistent with the bulk of the evidence, the proportion of brain and schizophrenic patients in our study is probably higher than in some studies, especially community surveys. At the same time, community surveys may actually underreport the number of physically based mental disorders. The Midtown Manhattan study reports a negative correlation between socioeconomic status and reports of poor physical health as well as between socioeconomic status and psychiatric symptoms. A positive relationship between reports of physical illness and rates of psychiatric disorder thought to be psychogenic is also indicated. Interpretation of these findings is problematic, of course, because psychophysiological symptoms form an important basis for psychiatric assessments (for a discussion of this problem, see Crandall and Dohrenwend 1967, pp. 1527–37). But, the psychiatric conditions may be due to the physical condition rather than vice versa. Indeed, Derogatis, Yeveseroff, and Wittlesberger believe their findings indicate that the relationship between socioeconomic status and psychiatric disorder may represent an extension of the relationship for physical disease (1975, p. 190).

Symptoms such as trembling hands and heart palpitations are treated in mental-health scales as physiological expressions of mental unease. It is at least as plausible to consider such symptoms as reflections of physical conditions. (In some instances, of course, they may be contributing factors to mental illness.) Thus, the lower socioeconomic strata may show a higher prevalence of mental illness in community studies only when mental illness is measured in terms of the number of psychophysiological symptoms. The position gains further credence from studies of socioeconomic status and mental illness that are based on the Langner scale, which may be weighted in favor of physiological symptoms (see Muller 1971, p. 601). Such findings may be confounded by either socioeconomic reactions to physical illness or the actual manifestation thereof (see Crandall and Dohrenwend 1967, p. 1535). In either case, data would indicate that the relationship between socioeconomic status and mental illness is, at least in part and perhaps for the most part, an aspect of a more general relationship between socioeconomic status and physical status.

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The Community Question: The Intimate Networks of East Yorkers[1]

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The Community Question has set the agenda for much of sociology. It is the question of how large-scale social systemic divisions of labor affect the organization and content of primary ties. Network analysis is proposed as a useful approach to the Community Question, because, by focusing on linkages, it avoids the a priori confinement of analysis to solidary groupings and territorial units. Three contentions about the Question are evaluated: arguments that Community is Lost, Saved, or Liberated. Data are presented about the structure and use of the "intimate" networks of 845 adult residents of East York, Toronto. Intimate networks are found to be prevalent, composed of both kin and nonkin, nonlocal, asymmetric, and of sparse density. Help in dealing with both emergencies and everyday matters is available from almost all intimate networks, but from only a minority of intimate ties. The data provide broad support for the Liberated argument, in conjunction with some portions of the Saved argument.

COMMUNITY AS NETWORK

The Community Question has set the agenda for much of sociology. It is the question of how large-scale social systemic divisions of labor affect the organization and content of primary ties. The Question thus has formed a crucial sociological nexus between macroscopic and microscopic analysis. It has posed the problem of the structural integration of a social system and the interpersonal means by which its members have access to scarce resources.

In considering the Community Question, sociologists have been espe-

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cially concerned with assessing the impact of industrialization and bureaucratization on a variety of primary ties: in the neighborhood, in kinship groups, in interest groups, and on the job. Urban sociologists, in particular, have been interested in this matter. From Tönnies ([1887] 1955) to Fischer (1977), they have investigated the effects of industrial bureaucratic social systems on communal structures, with particular reference to the following: (*a*) the increased scale of the nation-state's activities, with a concomitant low level of local community autonomy and solidarity (e.g., Tilly 1973, 1975); (*b*) the development of narrowly instrumental bureaucratic institutions for production and reproduction (cf. Castells 1976); (*c*) the large size of cities, with the consequent population and organizational potential for diverse interest groups; (*d*) the high social density of interactions among the segments of the population (even where spatial density is low), with the ensuing complexities of organizational and ecological sorting;[2] (*e*) the diversity of persons with whom city dwellers can come into contact under conditions of heightened mobility; and (*f*) widespread networks of cheap and efficient transportation facilities, letting contact be maintained with greater ease and over longer distances (cf. Meier 1968). The increased velocity of transactions facilitates interactional density: the large-scale city is accessible, centralized control can more effectively be imposed, and links to diverse social networks can more readily be maintained.

Unfortunately, in many community analyses, the basic structural concerns of the Community Question have become confounded with two other sociological concerns: (1) a preoccupation with the conditions under which solidary sentiments can be maintained, reflecting a continuing, overarching sociological concern with normative integration and consensus; and (2) a preoccupation with locating primary ties in local areas, reflecting urban sociology's particular concern with spatial distributions.

As a result of this confounding, the fundamentally structural Community Question has often been transmuted into a search for local solidarity, rather than a search for functioning primary ties, wherever located and however solidary. (It is my underlying argument that the proper concern of sociologists is the analysis of social structure and social linkages, with questions of social sentiments and spatial distribution holding important, but secondary, positions.) Analyses have tended to take as their starting point extrinsic mappings of local area boundaries and then proceeded to enquire into the extent of communal interaction and sentiment within these

[2] Early formulations of this point were in terms of high spatial density, but such formulations have been called into question by both the suburban dispersion and doubts about the social effects of crowding and density. In any event, analyses of spatial density tend to use interactional density as an intervening variable (see Freedman 1975), and if the questionable premise is not valid, the useful conclusion still remains (see Abu-Lughod 1969; Tilly 1970).

boundaries. They have thus assumed, a priori, that a significant portion of an urbanite's primary ties are organized by locality. Such a territorial perspective, searching for answers to the Community Question only within bounded population aggregates, has been especially sensitive to the evaluation of community solidarity in terms of shared values (see the discussions in Friedmann 1974; Howard 1974). Consequently, when there has been an observed dearth of locally organized solidary behavior and sentiments, the assumption has easily been made that "community" has decayed. Such assumed losses of community have been prevalent in the contemporary milieu of frequent residential mobility and spatially dispersed networks and activities.

Conceptualizing the interpersonal life of the city dweller as the central node linking together complex network structures leads to quite different analytic concerns from conceptualizing it as a membership in a discrete solidarity. Hence I propose an examination of the Community Question from a network analytic perspective. The utility of the network perspective is that it does not take as its starting point putative solidarities—local or kin—nor does it seek primarily to find and explain the persistence of solidary sentiments. It attempts to avoid individual-as-unit research perspectives, with their inherent social psychologistic explanatory bases, seeing internalized attitudes as determining social relations.

Instead, social network analysis is principally concerned with delineating structures of relationships and flows of activities. By looking directly at linkages rather than at solidarities, the network perspective enables us to focus directly on the basic structural issues posed by the Community Question.[3] Such an approach can do much to free the study of community from normative and spatial predilections.

This paper presents a social network analysis of the Community Question debate in urban sociology, as informed by a study of close ("intimate") ties in East York, Toronto. I first review three current Community Question arguments from a network analytic perspective: contentions that

[3] Although often mistakenly thought of as a collection of techniques, network analysis is essentially an analytic perspective which focuses on structured relationships between individuals and collectivities. As yet, there is no commonly agreed upon definition. Some of the salient characteristics of network analysis are that it gives attention to (*a*) structured patterns of relationships and not the aggregated characteristics of individual units analyzed without reference to their connectivity; (*b*) complex network structures and not just dyadic ties; (*c*) the allocation of scarce resources through concrete systems of power, dependency, and coordination; (*d*) network boundaries, clusters, and cross-linkages; and (*e*) complex structures of reciprocal relationships and not just symmetrical relationships or simple hierarchies. For summaries of the network perspective see Emerson (1962), White (1965), Mitchell (1969, 1974), Barnes (1972), Kemper (1972), Craven and Wellman (1973), and White, Boorman, and Breiger (1976). See also the bibliographies of Wellman and Whitaker (1974), Freeman (1976), and Klovdahl (1977).

Community is now "Lost," "Saved," or "Liberated" (the arguments are more fully discussed in Wellman and Leighton 1979). I then examine these arguments in the light of the data. Last, I discuss some of the broader implications of this review and the findings for the analysis of the Community Question.

COMMUNITY: LOST, SAVED, LIBERATED

Community Lost

The Lost argument was the first urban sociological response to the Community Question (e.g., Tönnies [1887] 1955), and it is still significantly influencing the current debate.[4] The argument holds many urban phenomena to be concrete and concentrated manifestations of industrial bureaucratic societies. It contends that the division of labor in these societies has attenuated communal solidarities. Primary relationships in the city now are "impersonal, transitory and segmental" (Wirth 1938, p. 12). Instead of being fully incorporated into a single solidary community, urbanites are seen as being limited members of multiple social networks, sparsely knit and loosely bounded. Their weak, narrowly defined, and disorganized ties are rarely available or useful for help in dealing with contingencies. Consequently, urbanites are now bound to the city by webs of secondary affiliations.

The Lost argument has occupied an important place in North American thought, from Jeffersonian antiurbanism through Progressive reformism (e.g., Woodsworth [1911] 1972) and "Chicago school" urban sociology (e.g., Park 1925*a*; Wirth 1938) to recent jeremiads against "mass society," both scholarly (e.g., Nisbet 1969) and popular (e.g., *Death Wish* [1974]). The argument's emphasis on the alleged disorganizing effects of attenuated communal solidarities has been reflected in substantive accounts of such diverse areas as collective action, crime, migration, poverty, and suburbia (see the critical reviews of Valentine 1968; Feagin 1973; Mostacci 1976).[5]

The Lost argument has usefully sharpened awareness of potential relationships between industrial bureaucratic divisions of labor and structures of primary ties. Yet, because of its assumption that strong primary ties naturally occur only in densely knit, self-contained solidarities, the argument has unduly neglected the question of whether primary ties have

[4] See the reviews of Stein (1960), Nisbet (1969), Gusfield (1975), and Castells (1976). Good examples of nonurban sociological uses of the Lost argument can be found in the political analyses of Kornhauser (e.g., 1968) and Gurr (e.g., 1969); see also Tilly's critiques (e.g., 1978).

[5] See White and White's (1962) and Marx's (1964) historical accounts of American antiurbanism.

been structurally transformed, rather than attenuated, in industrial bureaucratic social systems.

Community Saved

Many urban scholars have been dismayed by the Lost argument's emphasis on urban disorganization. In reaction, they have developed the Saved argument during the past 30 years, contending that neighborhood and kinship solidarities have continued to flourish in industrial bureaucratic social systems. The Saved argument asserts that such communal solidarities have persisted because of their continued efficacy in providing support and sociability, communal desires for informal social control, and ecological sorting into homogeneous residential and work areas (see Keller 1968; Suttles 1972). While granting that contemporary urban milieus also foster membership in more narrowly based multiple social networks, the Saved argument maintains that many of these networks tend to develop solidary features: single-stranded ties often broaden in scope as new aspects of the informal relationship develop (see Craven and Wellman 1973; Pickvance 1975), and densely knit, self-contained clusters of ties often emerge in initially sparse networks. Members of such networks are often important sources of assistance in mediating with formal bureaucratic structures and in coping with contingencies (e.g., Young and Wilmott 1957; Gans 1962, 1967; Liebow 1967; Stack 1974).[6]

Much of the Saved argument's case has rested on the sheer empirical demonstration of the continued vitality of those urban primary ties which had been pronounced Lost. Communal structures have been extensively documented in the Saved argument, in contrast with the Lost argument's analytic presentation of urbanites as aggregates of disconnected individuals. While some proponents of the Lost argument have alleged an association between communal disorganization and poverty (cf. Valentine's [1968] critique), those who have developed the Saved argument have found much evidence of solidary networks among poorer, traditional, or ethnic minorities seeking to maintain their resources against the claims of a centralizing state (cf. Tilly 1978). In the Saved argument, human beings are regarded as inherently gregarious, apt to organize communities under all circumstances. By the early nineteen sixties, the Saved argument had become the new orthodoxy, with the publication of such works as Gans's (1962) study of an "urban village," Greer's (1962) theoretical development of postwar survey research, and Jacobs's (1961) assertion of the vitality of dense, diverse central cities.

[6] There are clear similarities here to analyses of the importance of solidary ties in bureaucratic workplaces (e.g., Benyon 1973; Braverman 1974).

The problem of solidary ties.—Although the Lost argument's assertion of urban social disorganization has been rebutted, theoretically and empirically, this work has been accomplished by studies emphasizing the persistence of bounded communal solidarities. Such studies, while properly questioning the Lost argument's conclusions, have unfortunately not considered fully the Lost argument's useful starting point: that the contemporary division of labor may have strongly affected the structure of primary ties. Because Saved scholars have looked only for—and at—the persistence of communal solidarities in neighborhoods, in kinship systems, and on the job, it has been difficult to assess the position of solidary ties within overall social networks.[7] Weaker, more sparsely knit, more loosely bounded ties are all apt to be poorly represented in the Saved studies (see the discussion in Granovetter [1973]). While some Saved analyses have been quite concerned with external linkages, these linkages have been seen as radiating outward from a bounded communal base—often a small-scale territory or neighborhood (cf. Janowitz 1952; Greer 1962; Suttles 1972; Hunter 1975; Warren and Warren 1976; Warren 1978).

Thus the basic Community Question, dealing with the structure and use of primary ties, has been confounded in both the Lost and Saved arguments with questions about the persistence of solidary sentiments and territorial cohesiveness. But, whereas the Lost argument laments their demise, the Saved argument praises their persistence.

Community Liberated

The Liberated argument has developed out of the analytic juxtaposition of the Lost and Saved arguments. The Liberated argument affirms the prevalence and importance of primary ties but maintains that most ties are not now organized into densely knit, tightly bounded solidarities. The argument contends that: (*a*) the separation of residence, workplace, and kinship groups involves urbanites in multiple social networks with weak solidary attachments; (*b*) high rates of residential mobility weaken existing ties and retard the creation of strong new ones; (*c*) cheap, effective transportation and communication reduce the social costs of spatial distances, enabling the easy maintenance of dispersed primary ties; (*d*) the scale, density, and diversity of the city and the nation-state, in combination with widespread facilities for interaction, increase possibilities for access to loosely bounded, multiple social networks; and (*e*) the spatial dispersion of primary ties and the heterogeneity of the city make it less likely that those with whom an urbanite is linked will themselves be densely knit into solidary communities.

[7] Perhaps only Edward Banfield (1958) has gone out searching for solidary ties and not found any.

The Liberated argument has been systematically developed and tested only during the past 10 years.[8] Its take-off point has been that work of the Saved argument which has given analytic attention to urbanites' limited involvement in their local communities and to their external linkages beyond local boundaries (see also Merton 1957; Kasarda and Janowitz 1974; Taub et al. 1977). Taking this work further, the Liberated argument has abandoned the local area as the starting point for analyzing the Community Question and inquired directly into the structure of primary ties.

The Liberated argument contends that primary ties now tend to form sparsely knit, spatially dispersed, ramifying structures instead of being bound up within a single densely knit solidarity (see Kadushin 1966; Shulman 1972, 1976; Granovetter 1973; Laumann 1973; Breiger 1974; Shorter 1975; Fischer 1976; Walker 1977). While such ties may have fewer strands in the relationship than those in which kinship, residence, and work are combined, they are prevalent and important sources of sociability and support.

The argument suggests that primary ties are often dispersed among multiple, sparsely interconnected social networks. These networks, by their very nature, are not "institutionally complete" (Breton 1964), self-contained "urban villages." Their sparsely knit, ramifying structures provide a broad range of direct and indirect connections to the dispersed and differentiated resources of industrial bureaucratic social systems. Obtaining resources through such a sparsely knit network is not a matter of obligations due a member of a solidarity. Instead, it is a matter of the quality of the particular dyadic ties, the ease of maintaining contact, the ability of network members to provide indirect connections to additional resources, the extent to which additional members of a network can be mobilized to provide assistance, and the connectivity between networks (see Cohen 1969; Lee 1969; Bott 1971; Boissevain 1974; Granovetter 1974; Howard 1974; Walker 1974; Jacobson 1975; Fischer et al. 1977; Wireman 1978).

Answers and questions.—The Liberated argument has usefully freed the Community Question from its local roots. Yet a number of questions remain, because asserting that one should not set out initially to search for solidarities is quite different from asserting the nonexistence of such solidarities. First, to what extent do continuing kinship and local systems

[8] Some earlier scholars, who principally made the Lost argument, were also more optimistic at times about the consequences of this change in community structure. In their celebration of the potential for making choices among networks in the city, they prefigured the Liberated argument. For example, Georg Simmel contended that the urbanite, freed from a single encapsulating solidarity, had gained "freedom of movement . . . [and] a specific individuality to which the division of labor in the enlarged group gives both occasion and necessity . . ." ([1902–3] 1950, p. 417; see also [1908] 1971, p. 121). Robert Park's work (e.g., 1925*a*, [1925*b*] 1967) conveys a sense of excitement about the possibilities for individual action in the hurly-burly of the city.

structure primary ties? Second, are there no costs to maintaining ties over distance and no advantages to the quick physical accessibility afforded by proximity? Third, are there structural pressures toward the formation of solidarities, as friends of friends become friends of each other, as these increasingly dense clusters tend to interact more with each other and less across network boundaries, and as network members develop new strands in their relationships (see White 1965)? Fourth, are there circumstances—for example, lack of physical mobility or material resources, cultural differences—which can maintain dense, bounded solidarities? Fifth, is the maintenance of solidary sentiments dependent upon an unambiguous attachment to only one densely knit, tightly bounded communal structure?

Posing these questions is not to vitiate the Liberated argument, but to acknowledge that the formulation of the Community Question in network analytic terms has not only performed a useful critique of the Lost and Saved arguments but also provided us with a new structural perspective toward evaluating empirically some of their continuing concerns.

THE COMMUNITY QUESTION IN TORONTO

The foregoing analysis of the Community Question debate has developed concurrently with our research group's study of primary ties in Toronto. This research has been concerned with a number of issues which permeate the three arguments: To what extent are primary ties prevalent in industrial bureaucratic cities? To what extent is their composition based on kinship and neighborhood solidarities rather than on friendship? How homogeneous are urbanites' primary networks? How self-contained or ramified? How densely knit? What are the structural conditions associated with the availability of interpersonal assistance through these primary ties? The Lost, Saved, and Liberated arguments give quite different answers to these questions.

The Toronto research into such matters has been primarily survey based, supplemented by field work and focused interviews. The data discussed in this paper are derived from a 1968 random-sample survey of 845 adults (aged 18 and over) residing in the Toronto borough of East York. East York (1971 population = 104,645) is an upper-working-class/lower-middle-class, predominantly British-Canadian, inner suburb. Most residents live in small private houses or high-rise apartments; there are rarely more than two adults per household (see Gillies and Wellman 1968; Wellman 1976). East York has had the reputation of being one of the most solidary areas of Toronto. As such, it is a particularly interesting site at which to investigate the Community Question.

The survey asked respondents to provide detailed information about their six closest intimates ("the persons outside your home that you feel

closest to"), the ranked strength of closeness of their relationship with the respondent, their gender and socioeconomic status, the basis of their relationship (e.g., mother, neighbor), where they live, how often they are in contact (and by what means), and the kinds of assistance available in the relationships. Information was sought about the structure of these small egocentric intimate networks by inquiring into the respondents' reports of the interconnecting close ties among the sets of intimates named ($N =$ 3,930 intimates).[9]

Findings from this investigation of intimate ties will be presented in the next two sections. Despite the limitations of an analysis restricted to a quantitative case study of strong intimate ties, the data can help inform the Community Question debate. Research into the nature of primary ties is continuing in Toronto. Barry Leighton and I are now conducting in-depth reinterviews of a small subsample of the original respondents. A future monograph (Wellman, Shulman, Wayne, and Leighton, in preparation) will address such complementary matters as the nature of ties weaker than intimacy, the network dynamics of utilizing primary ties, longitudinal changes in primary networks (see also Crump 1977), and the relationship of solidary sentiments to network structures.

THE SOCIAL BASES OF INTIMACY

Relational Bases

Almost all (98%) East Yorkers report having at least one intimate tie; the majority (61%) report having five or more. Most have intimate ties with both kin and friends. For the sample taken as a whole, about half of all the intimate ties are with kin and about half are with unrelated individuals, predominantly "friends" who are not currently neighbors or co-workers (table 1).

The strongest intimate ties (in terms of the respondents' relative strength of closeness to these extrahousehold intimates) are usually with immediate kin (adult children, parents, and siblings), a traditional basis for solidary

[9] The questions to the respondents relevant for this paper were: "I'd like to ask you a few questions about the people outside your home that you feel closest to; these could be friends, neighbours or relatives. Please write in their initials, . . . with the one you feel closest to on the first line, the next closest on the second line, and so on. Will you now tell me the relationship to you of each person you have written down. . . . Now, for the first person listed, . . . Where does he/she live? How often do you *see* him/her? How often are you in touch by *phone* or *letter?* . . . Which of these do you rely on for help in everyday matters? Which of these do you rely on for help in an emergency? . . . [After six intimates:] I'd like to know which of the people . . . are close to one another. Tell me about the first one, please. Which of the others are close to that person? Which are close to Person 2? [etc.]" The data were originally collected in a study directed by Donald B. Coates, with Barry Wellman as co-director. See Coates et al. (1970, 1976). See also Wayne (1971).

TABLE 1

STRENGTH OF INTIMATE RELATIONSHIP BY RELATIONSHIP TO RESPONDENT

STRENGTH OF RELATIONSHIP (Ranked)	RELATIONSHIP								
	(All Kin)	Child	Parent	Sibling	Other Relative	Friend	Neighbor	Co-worker	TOTAL
1.	525	106	153	167	99	226	32	28	811
	(27.1)	(46.7)	(45.0)	(28.2)	(12.7)	(15.3)	(13.1)	(13.0)	(20.9)
2.	448	61	94	119	174	279	43	27	797
	(23.1)	(26.9)	(27.6)	(20.1)	(22.3)	(18.9)	(17.6)	(12.5)	(20.6)
3.	352	30	38	128	156	298	55	36	741
	(18.2)	(13.2)	(11.2)	(21.6)	(20.0)	(20.2)	(22.4)	(16.7)	(19.1)
4.	265	18	28	77	142	280	38	43	626
	(13.7)	(7.9)	(8.2)	(13.0)	(18.2)	(19.0)	(15.5)	(19.9)	(16.2)
5.	193	8	20	52	113	227	42	46	508
	(10.0)	(3.5)	(5.9)	(8.8)	(14.5)	(15.4)	(17.1)	(21.3)	(13.1)
6.	155	4	7	49	95	166	35	36	392
	(8.0)	(1.8)	(2.1)	(8.3)	(12.2)	(11.2)	(14.3)	(16.7)	(10.1)
N	1,938	227	340	592	779	1,476	245	216	3,875
% of total	50.0	5.9	8.8	15.3	20.1	38.1	6.3	5.6	100.0
Mean rank	2.8	2.0	2.1	2.8	3.4	3.3	3.5	3.7	...

NOTE.—$\chi^2 = 441.3$, $P < .001$; gamma = .27; nos. in parentheses are percentages; missing data = 55.

ties. Furthermore, when neighbors and co-workers are considered as intimates at all, the ties with them are likely to be comparatively weak (table 1).

Most East Yorkers specialize in one type of intimate relationship, kin or friend, but they also maintain one or two other types of intimate ties. A sizable minority are "superspecialists": 19% name only kin and 18% name only nonkin. Kin and nonkin intimates tend to be in different clusters of their intimate networks and not to have intimate ties with each other. All of an East Yorker's intimates, though, are indirectly tied to each other through the respondent; many may also have nonintimate direct connections with one another.

The multiple bases of the intimate ties (kinship, friendship, etc.) and the lack of direct connections between the relationally different intimates are in accord with the Liberated argument (see Laumann 1973; Verbrugge 1977; Fischer et al. 1977). Yet multiplicity does not mean equality. Most East Yorkers feel closer to kin than to unrelated intimates, and the greater number of their intimate ties tend to be bound up in one type of relationship.

The prevalence and importance of kinship ties is congruent with the Saved argument (e.g., Litwak 1960; Adams 1968; Klatzky 1971; Gordon 1977). However, in treating kinship systems as separate analytic entities, such Saved arguments may have underplayed the multiple bases of contemporary urban intimate networks. Our data suggest a synthesis of the Liberated and the Saved arguments: the variety of intimate ties potentially provides access to a more diverse array of resources, while heavy involvement with kin retains connections to a somewhat solidary system.

Spatial Expanse

The distribution of intimates' residences reveals that these strong primary ties of East Yorkers are situated in a broad field of interaction in Metropolitan Toronto and beyond. The great majority of East Yorkers' intimates live within Metropolitan Toronto, but only a small minority (13%) live in the same neighborhoods as their respondents (table 2). The metropolitan area thus bounds the effective field of interaction more than does the neighborhood. However, one-quarter of the intimates live outside Metropolitan Toronto, some as far away as Vancouver and New Delhi.

The distances at which intimate links are apt to be maintained vary markedly with the relational basis of the tie. Distant ties are much more likely to be with kin than with friends: 34% of intimate kin live outside Metropolitan Toronto, more than twice the percentage of unrelated intimates (table 2). Furthermore, ties with kin are the more actively main-

TABLE 2

RESIDENCE OF INTIMATES BY RELATIONSHIP TO RESPONDENT

Residence	Relationship							Total
	Child	Parent	Sibling	Other Relative	Friend	Neighbor	Co-worker	
Same neighborhood	9	23	25	54	194	182	18	505
	(4.0)	(6.8)	(4.2)	(6.9)	(13.1)	(74.3)	(8.3)	(13.0)
Elsewhere in East York	23	35	63	85	211	38	28	483
	(10.1)	(10.3)	(10.6)	(10.9)	(14.3)	(15.5)	(13.0)	(12.5)
City of Toronto	26	94	130	176	441	10	83	960
	(11.5)	(27.6)	(22.0)	(22.6)	(29.9)	(4.1)	(38.4)	(24.8)
Elsewhere in Metro Toronto	108	50	147	227	359	10	71	972
	(47.6)	(14.7)	(24.8)	(29.1)	(24.3)	(4.1)	(32.9)	(25.1)
Outside Metro Toronto	61	138	227	237	271	5	16	955
	(26.9)	(40.6)	(38.3)	(30.4)	(18.4)	(2.0)	(7.4)	(24.6)
N	227	340	592	779	1476	245	216	3875
% of total	5.9	8.8	15.3	20.1	38.1	6.3	5.6	100.0

NOTE.—$\chi^2 = 1203.0$, $P < .001$; gamma = $-.32$ (relationship ordered by mean closeness rank for kin and nonkin [see table 1]); nos. in parentheses are percentages.

tained distant intimate ties, with a much higher frequency of in-person and telephone interaction.

The wide spatial expanse of intimate networks is facilitated by the telephone. Indeed, telephone contact between intimates is usually more frequent than is in-person contact (table 3). The two modes of communication are generally complementary and not substitutive; it is quite rare for there to be a good deal of telephone conversation between intimates without there also being frequent in-person meetings.[10]

Perhaps the greater bandwidth of communication available through in-person meetings provides necessary information to reaffirm, reinforce, and readjust relationships maintained routinely by telephone. In no instance is an intimate tie sustained solely through telecommunications.

Distant ties.—Contemporary transportation and communication facilities have lessened, but not eliminated, the constraints of distance on maintaining intimate contact. Intimates who live far from East York tend to have a different relationship, having much less frequent telephone and

TABLE 3

Reliance on Different Modes of Contact, Controlled by Intimates' Residential Location (%)

Mode and Frequency of Contact	Residential Location: Same Neighborhood	Elsewhere in East York	City of Toronto	Elsewhere in Metro Toronto	Outside Metro Toronto
In person weekly or more often; telephone weekly or more often	51.9	53.4	43.0	38.8	6.8
In person weekly or more often; telephone twice per month or less	31.5	15.5	13.1	11.6	4.8
In person twice per month or less; telephone weekly or more often	4.8	9.5	20.9	20.8	21.8
In person twice per month or less; telephone twice per month or less	11.9	21.5	23.0	28.8	66.7
Total	100.1	99.9	100.0	100.0	100.1
N	505	483	975	984	947
χ^2 ($P<.001$)	31.0	95.2	85.9	123.8	53.5
Conditional gamma (in person by telephone for each residential location)	.61	.77	.56	.64	.63

Note.—Zero-order gamma (in person by telephone, uncontrolled by residential location) = .67; partial gamma (in person by telephone, controlled by residential location) = .62.

[10] One straightforward exception is that intimates living on the same block rarely use the telephone for contact.

in-person contact.[11] Their infrequent contact ratifies the tie, and a potential is retained for more intensive use when needed. The minority of those distant intimates who do interact frequently tend to maintain contact by telephone (table 3).

Clearly, many of the long-distance intimate ties are rather dormant in their actual functioning, maintained through infrequent contact and structural embeddedness (see also F. Katz 1966; P. Katz 1974). However, the very existence of these semidormant ties may usefully link East Yorkers to other connections. Furthermore, these are intimate ties and not just distant links to kin and friends. There is the memory of past interaction and the anticipation of future use. When necessary, the costs of distance can be overridden by an emergency, as when a respondent flew 2,100 miles to nurse a sick mother in Calgary although Sunday telephone calls had sufficed for the previous 10 years (see also Boswell's [1969] Zambian example).

Local ties.—The great majority of East Yorkers' intimate networks are not organized into local solidarities. Few have more than one intimate who resides in their own neighborhood.

Yet East York's pride in its local community ties is not without foundation. Although the borough contains less than 5% of Metropolitan Toronto's population, fully one-quarter of the respondents' intimate ties are to other East Yorkers, and the percentage is even higher for ties to intimates who are not kin (see table 2). Furthermore, many now-distant ties had local origins (Shulman 1972).

Most East Yorkers also have useful ties with neighbors, although these rarely reach the strength of intimacy. On the average, they talk with five neighbors regularly and visit in the homes of three (Gates, Stevens, and Wellman 1973). Such local ties are used for easy sociability and assistance when quick physical accessibility is an important consideration.

The data on the spatial expanse of intimacy provide support for a synthesis of the Liberated and the Saved arguments. East York is neither a Gansian "urban village" nor a "community without propinquity" (Webber 1963). While local ties are real and important, their importance comes from their being only a component of a diverse array of relationships. Intimate ties are organized into local solidarities even less often than they are into solidary kinship systems. Indeed, the car, the telephone, and the airplane help maintain many kinship ties. Yet space is still a constraint; there are distances for each tie at which the cost of keeping in contact becomes too great for it to remain viable.

[11] Indeed, the nature of the relationships may affect the spatial expanse of the tie, as when an aging mother decides to rent an apartment near her daughter.

Network Structure

Density.—The mean density of East Yorkers' intimate networks is 33%; that is, one-third of all possible intimate ties between respondents' intimates are actually reported to exist. Only one-fifth of the networks have a density greater than 50% (table 4), although many intimates are more weakly connected to each other (cf. Granovetter 1973). Thus, the great majority of respondents are not encapsulated within the bounds of one solidary group, but are linked through their intimates to multiple, not strongly connected, social networks. The prevalent sparse density supports the Liberated argument.[12]

There are significant clusters of density within networks, though. Kinship systems often foster close ties among members, and those intimate networks which are predominantly composed of kin tend to be more densely knit than the others (see table 4). Kin members of intimate networks also tend to form densely knit clusters within the rather sparse overall networks. Intimate friends, in contrast, tend either to be unconnected to other intimates or to be linked dyadically to them.

Reciprocity.—Shulman's associated study (1972, 1976) interviewed 198 of the intimates named by a subsample ($N = 71$) of our respondents and

TABLE 4

KIN AND FRIENDS IN INTIMATE NETWORKS BY DENSITY

Density of Networks (Grouped) (%)	N	%	Cumulative %	% Kin in Such Networks	% Friends in Such Networks
0–25	388	47.1	47.1	36.4[a]	53.2[a]
26–50	261	31.7	78.8	56.9	35.9
51–75	65	7.9	86.7	56.9	37.0
76–100	110	13.3	100.0	73.7	20.1
Total	824	...	...	49.5	42.1
$F(3,820)$	...	...	...	48.6*	36.6*

[a] Rows do not add to 100% because co-worker and neighbor intimates are not included.
* $P < .01$.

12 This is called "sparse density" (or "sparsely knit") because less than a majority of all possible interconnections actually appear. However, Harrison White has pointed out that, without standards for comparison, we have no firm theoretical or empirical grounds for expecting higher density, especially when studying strong ties of intimacy (personal communication). Jack Wayne, using the same procedure as employed here, found the density of reported ties between intimates in an inland Tanzanian area (Kigoma) to be 76% (personal communication). Ties between respondents and intimates have been excluded in the density calculations, as such ties exist by definition. Links were calculated symmetrically: if a respondent reported intimate no. 1 to be close to intimate no. 2, it was also assumed that he or she perceived intimate no. 2 to be close to intimate no. 1.

asked them, in turn, who their intimates were. Overall, only 36% of the surveyed intimates reciprocally named East Yorkers as their intimates. The closest intimates (those ranked first by mutual respondents) were markedly more likely to see each other as mutual intimates. Others acknowledged return ties to the East Yorkers but weaker ones than intimacy. They have intimate relationships but different ones from those of the East Yorkers. These ramifying, nonreciprocating ties are in keeping with the Liberated argument and argue against the Saved argument's notion of tightly bounded, mutually oriented solidarities.[13]

Ramifications.—Taken together, the variety of types of intimate ties, the sparse network density, and the often-unreciprocated character of intimate bonds strongly suggest a ramified, loosely bounded web of primary ties, rather than an aggregation of densely knit, tightly bounded solidary communities. Only a minority of an East Yorker's intimates reciprocate intimacy, and only a minority of intimates are reported to be intimate with each other. The overall structure of intimate relationships is in accord with the Liberated argument.

Yet the data also indicate some basis for the closer structural integration suggested by the Saved argument. There are often dense clusters within more sparsely knit networks. Furthermore, many of those who are not intimately connected with each other are linked together in other important ways: as friends, acquaintances, neighbors, co-workers, and nonintimate kin. Using such less restrictive criteria, there is much structural connectivity.

THE AVAILABILITY OF HELP FROM INTIMATES

If East Yorkers are to avoid the direct dependence on formal bureaucratic resources seen by the Lost argument, they must be able to obtain assistance through their primary ties. Although such assistance might come through many ties, it is reasonable to expect that much reliance would be placed on help from intimates, the people outside the household to whom they feel closest.[14]

13 Shulman's findings (1972, 1976) indicate that the symmetry assumption may well overstate the density of the networks when only intimate ties are considered, although we wonder if the respondents would have perceived the asymmetry present in the ties between their intimates. This lack of reciprocity gives a structural basis for expecting wide disparities in the extent to which an urbanite is chosen as an intimate (see Rapoport and Horvath's [1961] study of a biased friendship network). This, in turn, indicates the structural prevalence of "brokerage" nodes, whose heavily chosen incumbents link together a number of social networks.

14 Our in-depth interviews indicate that respondents often perceive their intimate connections as a type of general utility. While they know that they might need help from intimates at some time, and maintain their ties in part for that purpose, often they do not have any precise idea of what contingencies will in fact develop. The treatment

The structural situation of East Yorkers, linked to intimates by means of sparse networks rather than solidary groups, is reflected in the nature of the help reportedly available. In support of the Liberated argument, we find that the great majority of East Yorkers (81%) report that help in emergency situations is available to them from somewhere in their intimate network. A smaller majority (60%) report help to be available through their intimate networks in dealing with everyday matters; such routine help is often available as part of less intensive relationships (e.g., with acquaintances, neighbors, co-workers), and there is less use of intimate relations for it.

While assistance in emergencies is available from the great majority of intimate networks, it is not available from the majority of intimate relationships. Only a minority of intimates, 30%, help in emergencies, and only 22% help in dealing with everyday matters. Thus, East Yorkers can almost always count on help from at least one of their intimates, but they cannot count on such help from most of them.

To some extent these data are consistent with the Lost argument's concerns about the attenuation of supportive primary ties. However, the data support more fully a differentiated conceptualization of intimacy, consistent with the Liberated argument's analysis of the division of labor in primary networks. Intimacy (or closeness) is not a unidimensional construct. "Helping" is a defining attribute of a minority of intimate relationships, while others may be based on sociability, structural or normative obligation, or propinquity (see Leyton 1974). The remainder of this section examines the effect of relational and structural factors in these differentiated networks on the likelihood that a respondent regards an intimate as a provider of assistance. Two path diagrams summarize the interrelationships; table 5 presents the correlation matrix for both diagrams.[15]

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of help as a generalized resource is a conservative estimate of its availability from intimates. East Yorkers may count on help from some other intimates for specific contingencies, defined by the relationship and the resources available, while not thinking of these intimates as being generally helpful. It is the generalized role relationship of "helper," clearly evident in our in-depth interviews and field work, that is of interest here. Such membership in general-purpose helping relationships challenges a market model of assistance, in which a seeker rationally determines a need, scans all available sources, and calls upon them in ranked order of probable utility. Not only is the provision of help determined by networks, but so may be the perception and utilization of available channels. Indeed, the very provision of help may precede—and define—the putative seeker's desire to enter into a help-receiving relationship.

[15] Earlier analyses (e.g., Wellman et al. 1971) indicate that there are no appreciable direct associations between such social categorical variables as age and SES and the availability of help. They thus were omitted from the analysis for this paper. See Wellman (1977) for cross-tabulations presenting more detailed information about the relationships between the network variables and the availability of help.

TABLE 5

ZERO-ORDER CORRELATION MATRIX FOR PATH DIAGRAMS

	Parent/ Child	Lives inside Metro Toronto	Co-worker	Centrality	In-Person Contact	Closeness Strength	Telephone Contact	Emergency Assistance
Lives inside Metro Toronto	−.100							
Co-worker (fig. 2 only)	−.102	.101						
Centrality	.236	−.079	−.092					
In-person contact	−.028	.346	.437	−.012				
Closeness strength	.267	−.011	−.098	.044	.035			
Telephone contact	.205	.282	.008	.098	.337	.212		
Emergency assistance from intimate (fig. 1)	.180	.105	−.021	.054	.147	.280	.197	
Everyday assistance from intimate (fig. 2)	.112	.134	.093	.065	.202	.221	.248	.444

Kinship

The most antecedent variable in the path models traces the continuing effect of kinship ties. The role of the extended family as a special provider of assistance is confined among East Yorkers to intimate parents and (adult) children. Other intimate kin, such as siblings, grandparents, and aunts, are only as likely as friends to provide assistance.

Parent-child support is more marked in emergencies: 50% of parent-child ties have an emergency-assistance component, as compared with 26% of other intimate ties. Parents and children are more apt than other intimates to be called upon for help in emergencies, regardless of where they live (they tend to live at greater distances from respondents than other intimates) and how frequently they are in face-to-face contact (fig. 1). In addition to the direct effect, intimate parents and children are also more likely than other intimates to provide help, because they tend to have closer ties with the East Yorkers and to be in more frequent contact with them.

While parent-child intimates are also significantly more likely (34%) than all others (19%) except co-workers (37%) to help out with everyday affairs (Wellman 1977), there is no direct effect on kinship in this case (fig. 2). There are indirect effects, however, due to the stronger bonds and more frequent telephone contact that parent-child intimates have.

The kinship data partially support both the Saved and the Liberated models. Kinship remains a significant basis for providing help, both directly and because it encourages closer bonds and more frequent telephone contact. Yet the particularly helpful intimates are parents and children and not a large solidary network of extended kin relations.

Propinquity

Our earlier analyses (Wellman 1977) showed the availability of assistance to be not significantly associated with intimates' neighborhood residence, in contradistinction to the Lost and Saved arguments' emphases on local solidarities. Accordingly, the local residence variable has been omitted from the final path analyses.

Proximity appears to be more important on the job than in the neighborhood for the availability of help from intimates. Co-workers' frequent face-to-face contacts make them a significant source of everyday assistance for East Yorkers, despite the comparative weakness of their intimate bonds.

The residential distinction that does make a difference in the availability of help is that of living inside Metropolitan Toronto's boundaries; that is, being a local call or a short drive away. This has a slight direct positive effect on the availability of help and appreciably increases the fre-

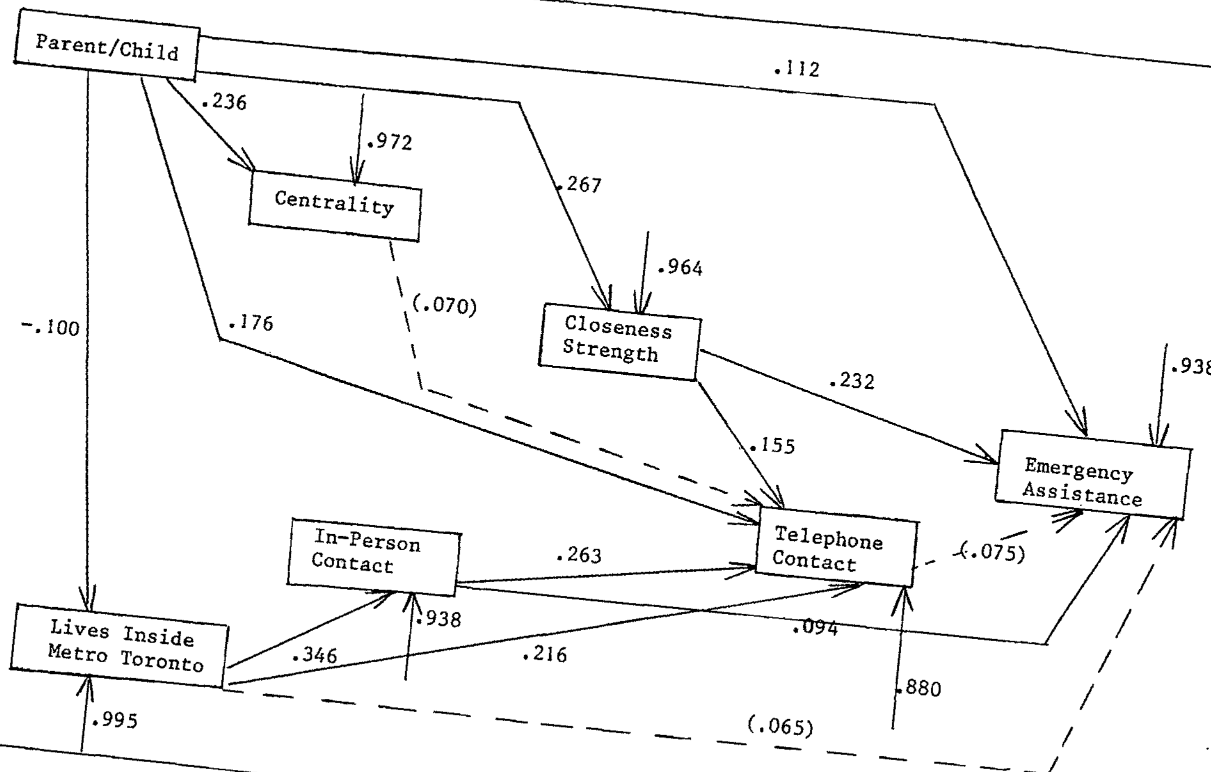

FIG. 1.—Path model: emergency assistance from intimates. Dashed lines with correlations in parentheses represent nonsignificant paths necessary to reproduce original correlation matrix to .05.

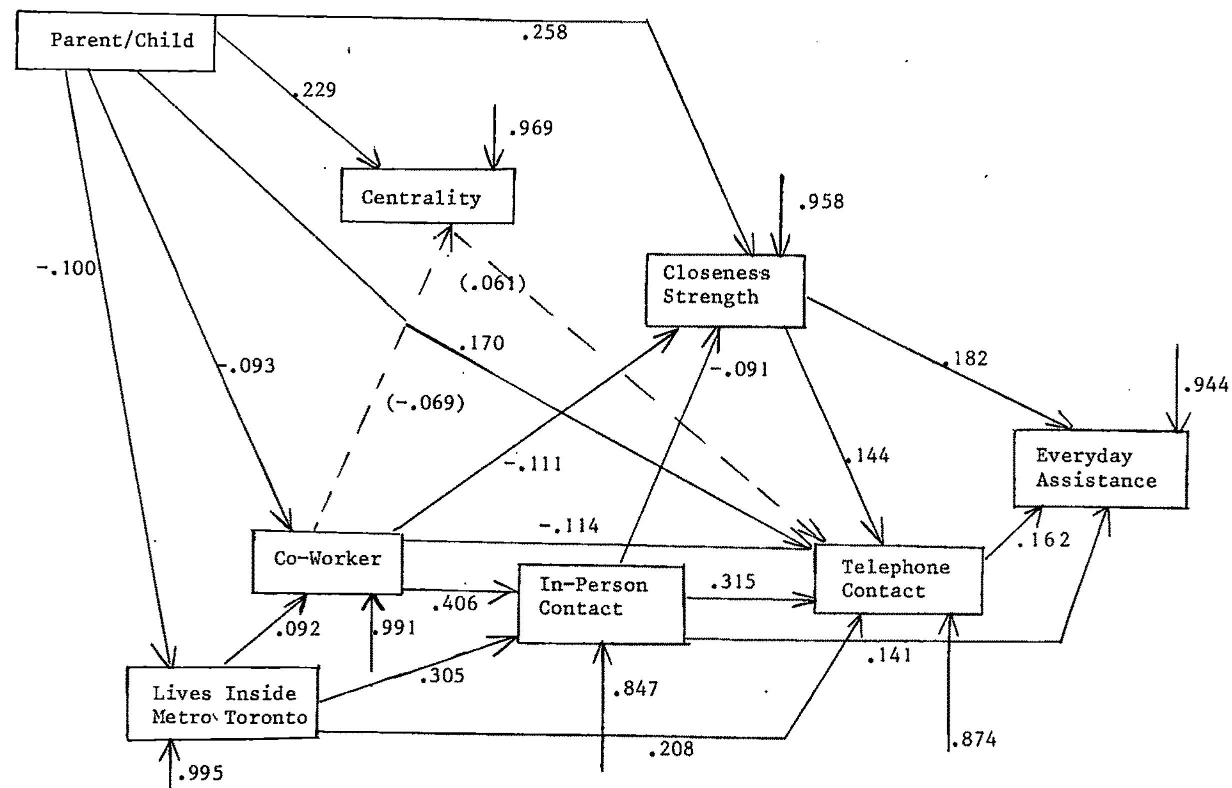

FIG. 2.—Path model: everyday assistance from intimates. Dashed lines with correlations in parentheses represent nonsignificant paths necessary to reproduce original correlation matrix to .05.

quency of contact between intimates. (There may be reverse effects operating as well, with intimates choosing to live in Metropolitan Toronto so that they may continue to be available to help their East York respondents.) The data support a somewhat revised version of the Liberated argument: to an appreciable extent, the spatial range of assistance relationships has not disappeared, but has expanded to encompass the entire metropolitan area.

Centrality and Density

One purely structural variable, an intimate's centrality in a respondent's network (measured as the number of intimate ties that an intimate has with any of the respondent's other intimates), slightly affects the frequency of telephone contact and, hence, the provision of assistance. In general, more structurally central intimates are more likely to provide help. Indeed, their ability to provide help may have made them central.

A structurally central person's potential ability to mobilize help is not related to the solidary nature of the network: no significant paths between the density of a respondent's network of intimates and the availability of assistance from an intimate have been found in our analyses. Hence density has been deleted from the final path models. The absence of significant density effects and the weak effects of centrality also argue that the helpfulness of parents and children is independent of the potential solidarity of their kinship networks. It is a component of dyadic parent-child relations. In sum, the centrality and density data support the Liberated argument better than the Saved argument.

Frequency of Contact

The more frequently intimates are in contact, especially in person, the more apt are they to provide assistance in their relationships.[16] Frequent contact is particularly associated with the more mundane provision of everyday assistance, when ready accessibility is more likely to be a mobilizing factor.

Closeness

The closer (stronger) the intimate relationship (as measured by the respondent's ordinal ranking of the intimates), the more the perceived

[16] For the path analyses, the original categorically recorded frequency-of-contact data were transformed into estimated-days-per-year equivalents. E.g., "about once a week" was transformed into "52." This transformation makes the simplifying assumption of equal time spent per contact.

availability of help becomes a salient defining component of that tie. Closeness is apparently the single most important defining characteristic of helpful intimate relationships; it is the strongest direct predictor in the path models.[17] For example, 56% of the first closest ranked intimates are relied on in emergencies by East Yorkers, while only 16% of the sixth closest intimates are. Closeness also has appreciable paths to the next most powerful predictors, the frequency of contact variables. Furthermore, all other significant variables predict to it, directly or indirectly.

The data indicate that the availability of help to East Yorkers from intimates is a process more fully in accord with the Liberated argument than with the Saved argument. The full path diagrams show two social processes, both more closely associated with the nature of two-person intimate bonds than with the structure of overall intimate networks. On the one hand, a comparatively strong "interactional" set of paths go from the spatially propinquitous facilitation of interaction (through living in the same metropolitan area or being a co-worker) to the frequency of interaction to the availability of assistance. On the other hand, another set of "familial" paths go from parent-child ties to the strength of closeness of intimate ties to the availability of assistance.

The availability of the parent-child tie for assistance is not associated with the tie being embedded in strong, supportive kinship relationships. Furthermore, structural variables, such as centrality and density, are poorly related to the availabiilty of assistance. The availability of assistance thus is more closely associated with the character of the two-person bond than it is with the potentially solidary character of the overall network.

Although the data document network effects on the availability of help from intimates, the amounts of explained variance in the path models are not large. I am reluctant to relinquish most of the unexplained variance to unspecified, residual "psychological factors." Some of the unexplained variance is probably due to the crude way in which the variables have been defined and measured. Furthermore, the way is surely open to the delineation of additional structural and categorical variables that can affect the interpersonal provision of scarce resources.

COMMUNITY IN EAST YORK: LOST, SAVED, OR LIBERATED?

Community Lost?—The prevalence of strong intimate ties in East York calls into question the basic contention of the Lost argument (see summary table 6). If kin and neighbors have been lost as intimates, they apparently

[17] We are concentrating here on predicting to the reported availability of help. But it is also quite likely that, reciprocally, being perceived as helpful may engender stronger perceptions of closeness among intimates.

TABLE 6

THE COMMUNITY QUESTION: LOST, SAVED, AND LIBERATED ARGUMENTS COMPARED WITH EAST YORK FINDINGS

Argument	Community Lost	Community Saved	Community Liberated	East York Findings (Main Tendencies)
Basis of intimacy:				
Availability	Rare	Abundant	Abundant	5+ intimates
Relational	Formal role	Kin, neighborhood	Friendship, work	Kin, friendship
Spatial	Local	Local	Metropolitan, national	Metropolitan
Mode of contact	In person	In person	In person, telephone	Telephone, in person
Communal structure:				
Density	Sparse	Dense	Sparse	Sparse
Reciprocity	No	Yes	Uneven	Uneven
Boundedness	Ramified	Tight	Ramified	Ramified
Basis of assistance:				
Prevalence	Minimal	Abundant	Moderate	Moderate
Relational source	Formal ties	Kin, neighborhood	Friendship, work	Parent/child, work
Residential basis	Local*	Local	Metropolitan, national	Metropolitan
Density	Dense*	Dense	Sparse	N.S.
Structural source	Secondary	Solidary group	Network ties	Network ties

* To the extent to which primary ties exist.

have been replaced by friends and co-workers. Yet East Yorkers report that they can count on only a minority of their intimates for help. Communal networks of mutually supportive intimate relationships do not appear. If highly supportive communities ever did exist for East Yorkers, intimate ties now occur only as much more differentiated networks. However, in these networks, many intimate ties contain support as an important strand in the relationship, and help from intimates is available to almost all East Yorkers.

Community Saved?—The data support some aspects of the Saved argument, albeit greatly affected by the contemporary context (see summary table 6). Parent-child ties play a special role in the overall intimate networks. They tend to be socially closer than other intimate ties, even at greater physical distances. Parents and children are more apt to provide help in mundane matters as well as in crises. Other intimate kin, however, can be counted on no more than can intimate friends. Clearly, the important kinship obligations that most intimate parents and children maintain operate as dyadic relationships, as the data also indicate that the density of the network is not a factor in the mobilization of assistance. There are few large solidary networks of helpful kin.

Residential propinquity still facilitates the provision of assistance, but

the local area is now metropolitan and not the neighborhood. This implies that it is the physical availability of aid—by automobile, public transit, and telephone—which is operative and not the activity of neighborhood solidarities. Kinship and metropolitan residence both act to encourage frequent contact. Those in contact more often are more likely to feel closer and to provide assistance when needed.

Neighborly relations are prevalent and, for many East Yorkers, important. Considered separately, they validate the opinion East Yorkers have of themselves as being heavily involved in local community interactions. Yet the data indicate that such neighborhood ties are usually just one component of a more diverse set of relationships and that they rarely comprise the more intense intimate relationships. It may be that in other populations, less mobile or less preoccupied with controlling internal resources, a larger proportion of the relationships will be tightly bound in solidary groups (see Wolf 1966; Wellman and Leighton 1979). Clearly, more work needs to be done on the dynamics of establishing, maintaining, using, transforming, and losing primary ties.

Community Liberated?—Our findings most fully support the Liberated argument that East Yorkers tend to organize their intimate relationships as differentiated networks and not as solidarities. There is much differentiation in the nature and use of intimate ties. There are links to a variety of people with different structural positions, often living in quite different residential areas (or interacting at work), and maintaining contact both by telephone and in person, at a wide range of time intervals.

The availability of assistance is affected by the quality of the relationships and not by the extent of structural solidarity. Not all intimate ties are used similarly, even those which are densely knit. Some intimates can be counted on to provide assistance in dealing with everyday matters; a good many more, but certainly not all, give assistance when emergencies arise. Other intimates interact with East Yorkers on different bases, such as kinship obligations, sociability, or job comradeship.

The many components of intimate relationships are not very neatly associated; the "role frame" of intimacy includes many complexly packaged bundles of relationships (see Nadel 1957). Some intimate friends are seen only socially; some provide help routinely. Some intimate kin can be counted on in any emergency; others cannot. Some intimate kin are seen daily; some intimate friends, yearly; and so on. This lack of neat coincidence among the qualitative aspects of the relationships makes for more differentiated linkages (see Mitchell 1969; Litwak and Szelenyi 1969; Gordon 1976). Different contingencies, social situations, and times of the day, week, or year bring East Yorkers into juxtaposition with a variety of connections.

The data analyzed here may help to resolve the discrepancy between the

Saved argument's extensive documentation of solidarities and the Liberated argument's portrayal of differentiated networks. The resolution may be a matter of analytic scope. If one focuses on kinship systems or neighborly relations, one is apt to find densely knit, tightly bounded networks. Looked at in fine-grain isolation, these networks appear as solidarities, which may well serve to give urbanites a sense of attachment in the social system. But if one broadens one's field of view to include all those with whom an urbanite is in touch, then the apparent solidarities may be seen as clusters in rather sparsely knit, loosely bounded networks.

COMMUNITY: SOLIDARITY OR NETWORK?

Intimate networks are just one of a number of often quite distinct personal networks. Frequently, weaker ties, such as neighboring and coworking, have limits on the claims that can be made on them. But they also tend to provide indirect access to a greater diversity of resources than do stronger, more socially homogeneous ties (Granovetter 1973).

All persons with whom one is directly connected are indirectly linked to each other through oneself. Each individual is a member of the unique personal networks of all of the people with whom he or she is linked, and membership in these networks serves to connect a number of social circles (see Craven and Wellman 1973). Thus, complex networks of chains and clusters are ultimately connected via a common network node. Social solidarity, analyzed from this perspective, may be the outgrowth of the coordination of activities through network processes rather than of the sharing of sentiments through common socialization.

While making for low communal solidarity, a variety of ties and uneven network density provide structural bases for dealing with contingencies. Densely knit network clusters can provide the basis for cooperative activities. Ramifying networks and asymmetrically reciprocated linkages can facilitate access to other social circles.

The concatenation of networks helps to organize social systems. Considered from the standpoint of the system rather than from that of the individual, it is the compounding of links and networks at many levels which allocates resources and juxtaposes alliances of similar interests. Not only individuals, but also clusters and collectivities, are linked through network ties (cf. Granovetter 1976; Rytina 1977; Laumann, Galaskiewicz, and Marsden 1978). A network of networks connects individuals, clusters, and collectivities in complex ways.

Despite all this connectivity, our data also suggest why so many urbanites believe in the Lost argument, even when they themselves are well connected. Rather than an unambiguous membership in a single, almost concrete, solidary community, East Yorkers' lives are now divided among

multiple networks. The sparseness of interconnections among those networks means that no one solidarity can readily make or enforce general claims on a member. While this may be somewhat liberating in providing structural room to maneuver, it may also create a disorientating loss of identity, as it is no longer as clear or simple to which group (among many) one belongs. Although urbanites have not lost their communal access to people and resources—and, indeed, may have increased their reach—for those who seek solidarity in tidy, simple hierarchical group structures, there may now be a lost sense of community.

Yet membership in spatially and socially ramified networks is a useful way for urbanites to have access to diverse and differentiated resources not available through solidary auspices. Their ties are not encapsulated in "decoupled" little worlds (White 1966) but are strands in the larger metropolitan web. The ties provide the basis for network members to utilize the connections with others that their alters have. This suggests that Liberated networks may be more than just a passive rearrangement of primary ties in response to the pressures of large-scale social system changes. Instead, they can well be active attempts by contemporary urbanites to gain access to and to control system resources, given differentiated social systemic divisions of labor.[18]

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18 The discussion in this paper has been limited to the contemporary industrial bureaucratic situation, and no attempt has been made to address complex cross-cultural and longitudinal issues. Yet a developmental perspective that sees industrialization, bureaucratization, and urbanization as inevitably fostering communal transitions from Saved or Lost structures is quite questionable. It is entirely possible that Liberated networks exist in nonindustrial milieus under appropriate structural conditions of resource access and control. On the one hand, recent studies clearly indicate that differentiated long-distance ties are maintained in the Third World without the prevalence of telephones or private automobiles as long as the ties are structurally embedded in kinship systems or common local origins. A number of mechanisms are used to maintain contact, such as trips by buses and trucks, messages sent with covillagers or travelers, and hired letter writers and readers (e.g., Cohen 1969; Jacobson 1973; Howard 1974; Mayer and Mayer 1974; Ross and Weisner 1977; Weisner 1973). On the other hand, historians have been reporting nonsolidary aspects of many preindustrial Western European and North American communities, with status-group cleavages, appreciable spatial mobility, and complex households having heterogeneous external ties (e.g., Laslett 1971; Scott and Tilly 1975; Shorter 1975; Tilly 1975; Bender 1978).

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Differences in the Canadian and American Class Vote: Fact or Pseudofact?[1]

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This paper reassesses Alford's comparative analyses of class voting in Canada and the United States and updates them to 1976. It is argued that Alford's finding that the class vote in Canada is substantially lower than that in the United States is an artifact of his classification scheme, which incorrectly superimposes the American party structure on Canadian political parties. When Canadian political parties are classified according to the major party/third party division which more accurately reflects the realities of the "democratic class struggle" in Canada and when the correct techniques for measuring class voting are applied, it is shown that class voting in the two countries is virtually identical. It is not the level of class voting which distinguishes the two societies but rather the differential manner in which the democratic class struggle in each has been organized as a result of their different electoral systems.

The traditionally weak link between social class and electoral politics in the United States has made the "American exception" the subject of considerable speculation and empirical research among analysts of the "democratic class struggle" in Western societies. However, since the appearance of Alford's (1963, 1967) seminal analyses of class voting in the Anglo-American democracies, it has been accepted that while class voting in the United States is low, in Canada it is almost nonexistent. This paper will demonstrate that the apparently more marked "Canadian exception" is a pseudofact resulting from both theoretical and methodological problems associated with the analysis of class voting in Canada.

On theoretical grounds, there is little reason to expect a large difference in the class vote in the two countries since, as Alford himself noted (1963, p. 101), Canada and the United States are remarkably similar with respect to those factors usually called upon to explain a low level of class struggle. Where the two countries do differ, however, is the manner in which their respective electoral systems have mediated and organized this struggle. In

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the United States, class-based protest movements have largely been absorbed within the existing two-party structure. In particular, the Democrats, under Roosevelt, successfully became identified as the representatives of labor and hence as the party of the left. In Canada, on the other hand, no such alignment of a major party with working-class interests occurred. The Canadian parliamentary system has discouraged the absorption of class and sectional protest movements into the two major parties while actively encouraging their expression in the formation and institutionalization of third parties (Lipset 1976). The result is that Alford's classification of the Liberals and the New Democratic Party (until 1961 the Cooperative Commonwealth Federation [CCF/NDP]) on the left and Conservatives and Social Credit on the right is highly misleading. The left-right division of the two major parties, Liberals and Conservatives, in a manner analogous to the Democrat-Republican split in the United States, is particularly problematic.

In his review of the problem of classifying Canadian political parties, Ogmundson (1975) notes that the original classification proposed by Alford does not correspond to voter perceptions of the class interests represented by the different parties, their bases of electoral support, the social origins of their elected members, or their sources of party financing. On all of these indicators, the two major parties (Liberals and Conservatives) are clearly representative of the middle class while the two minor parties (CCF/NDP and Social Credit) are representative of the working class. Accordingly, he argues that estimation of the class vote in Canada should be based on a classification which distinguishes between the two major parties on the one hand and the two minor parties on the other. This problem of conceptual equivalence, however, is further compounded by the procedure used to measure the class vote.

MEASURING THE CLASS VOTE

Alford defines the class vote as the ". . . extent to which manual and non-manual strata divide in their support for political parties" (1963, p. 79). His index of class voting is computed by ". . . subtracting the percentage of persons in non-manual occupations voting for Left parties from the percentage of persons in manual occupations voting for Left parties" (1963, pp. 79–80). Alford's index is, in fact, identical with Somer's *d,* which in turn is identical with the slope of the regression of vote on class in the 2×2 table (see Korpi 1972, p. 631). This use of the regression approach when the dependent variable is a dichotomy has been criticized for various reasons (Goodman 1972; Theil 1970). For our purposes, the major difficulty is that the index is sensitive to the marginal distributions which generate the cell values and, hence, potentially confounds the effect of class

on vote within societies with differences in the overall level of right or left voting between societies. Such a property is undesirable since, as Alford observed, ". . . it is the gap between the voting patterns of manual and non-manual occupations which is vital for assessing class voting, not the over-all level of Right or Left voting" (1963, p. 80). Given the distributions in Alford's original analysis, this difficulty did not actually arise. However, when the classification of parties suggested above for Canada is employed, it becomes severe since the distribution of the dependent variable is highly skewed.

A strategy which overcomes the difficulties inherent in the usual regression approach is the use of odds ratio based measures which are insensitive to variations in the marginals. Several such approaches are available, each of which will provide equivalent results. One such method is the application of a modified multiple regression procedure based on log-linear techniques, as recently elaborated by Goodman (1972). An equivalent solution, which provides results more readily comparable with Alford's analysis, is to remove the effects due to marginal distributions by means of Deming adjustment (Deming 1943). This procedure involves the progressive standardization of rows and columns to a common set of marginals for all tables. It preserves the association between row and column variables (the relative odds ratio) while removing the differences due to the effects of the column and row marginals (Mosteller 1968). Following Mosteller's (1968) example, each 2×2 table (vote X class) included in the analysis was standardized on row and column marginals set equal to 100, and a standardized index was then computed in the usual way. This procedure was applied to all except two of the election polls originally utilized by Alford (1963, appendix B) as well as to two polls conducted in conjunction with each national election in Canada and the United States since 1961.[2]

RESULTS

The results from the reanalysis of Alford's (1963) data and the results for subsequent federal elections in Canada and the United States are presented in the Appendix. Inspection of the adjusted indices indicates a similar pattern for the two countries. The more recent results are summarized in table 1, which replicates and updates Alford's (1963, p. 102) summary table 5-2. Two sets of measures are presented for Canada: those computed when Alford's original classification of parties is used (Canada 1) and those computed using the major party/third party split (Canada 2). In each case two values are presented: the adjusted index of class voting

[2] The two polls excluded are American polls for 1940 and 1944 (sample 1) for which sample sizes were not provided and hence cell values could not be computed. Details on the selection of polls are available in the lengthier version of this paper available on request from the author.

TABLE 1

CLASS VOTING IN CANADA AND THE UNITED STATES, 1952–61 AND 1962–76

	1952–61						1962–76					
	Mean		Lowest		Highest		Mean		Lowest		Highest	
	A^*	A	A^*	A	A^*	A	A^*	A	A^*	A	A^*	A
United States	17	16	14	13	24	23	15	14	6	5	20	18
Canada 1	8	8	−2	−1	18	17	1	1	−6	−6	10	9
Canada 2	17	9	10	5	26	12	16	11	10	2	22	18

NOTE.—A^* = adjusted index; A = unadjusted index; Canada 1 = CCF/NDP and Liberals classified as left; Canada 2 = CCF/NDP and Social Credit classified as left.

after differences due to row and column marginals are removed (denoted by A^*) and the original indices as computed by Alford (denoted by A).

For the period 1952–61, it is evident from an examination of rows 1 and 2 that standardization has little effect on Alford's original indices. However, examination of row 3 indicates that when the major party/third party split is employed the unstandardized indices seriously underestimate the effect of class on vote in Canada. Moreover, it is clear that the size of the class vote in Canada and the United States is virtually identical over this time period. The mean for both countries is identical, while the range in Canada is slightly greater than that in the United States at both extremes.

For the more recent period, the consequences of reclassifying the Canadian parties are even more striking. Using Alford's original classification (Canada 1), the average class vote for Canada is almost zero, reflecting the fluctuation between low positive scores and low negative scores which appear in the Canadian data. One could only conclude from such results that class voting has declined in Canada over the last decade and a half to the point of being nonexistent. The results from the major party/third party classification (Canada 2), however, indicate a very different picture. Rather than declining, the level of class voting in Canada has remained constant. Once again, the Canadian average for the period is virtually identical with that for the United States.

CONCLUSION

In both Canada and the United States, the representatives of the working class are constrained in their pursuit of pure class political goals. Within the American context, the labor wing of the Democratic party is but one faction among several which must be appeased through coalition politics. In Canada, third-party representatives of the working class must rest content with the role of critic and proponent of policies which may or may not be adopted by the party in power. Whether coalition or third-party politics provides a more effective means of achieving (or suppressing) working-class interests is a topic which lies beyond the scope of this paper. For purposes of comparative political analysis, however, it is clear that future research should address the North American exception and that the Canadian exception is a pseudofact which can be laid to rest.

APPENDIX—Indices of Class Voting in Canada and the United States, 1936–76

	A^*			A		
Year	Canada 1	Canada 2	United States	Canada 1	Canada 2	United States
1936....	...	...	16, 16	...	...	16, 15
1940....	8	18	...	6	6	...
1944....	...	...	14, 21	...	...	14, 21
1945....	6, 8	22, 16	...	6, 8	16, 12	...
1948....	...	...	20, 30, 43	...	...	20, 29, 41
1949....	4	30	...	4	17	...
1951....	10	28	...	9	15	...
1952....	...	...	24, 18	...	...	23, 17
1953....	8	18	...	8	10	...
1954....	8	20	...	8	12	...
1956....	2	14	14, 16	2	8	13, 16
1957....	10, 10	12, 16	...	9, 10	6, 8	...
1958....	16	26	...	16	9	...
1959....	−2	16	...	−1	8	...
1960....	6	16	16, 14	7	8	15, 14
1961....	18, 4, 8	22, 20, 10	...	17, 4, 8	12, 12, 5	...
1962....	2, 6	22, 14	...	1, 6	18, 12	...
1963....	−4, −6	16, 16	...	−3, −6	13, 12	...
1964....	...	...	16, 20	...	...	14, 17
1965....	4, 0	10, 13	...	3, 1	2, 12	...
1968....	−6, 2	18, 20	18, 14	−5, 2	12, 14	17, 13
1972....	4, −6	12, 8	6, 16	3, −5	9, 7	5, 15
1974....	10, 2	16, 22	...	9, 2	11, 14	...
1976....	...	...	18, 14	...	...	18, 14

Sources.—Indices for 1936–61 recomputed from Alford 1963, appendix B. Indices for 1962–76 computed from the following sources. Canada: 1962: Canadian Institute of Public Opinion (CIPO) 296, 297; 1963: CIPO 301, 302; 1965: CIPO 315, 1965 Canadian National Election Study; 1968: CIPO 329, 330; 1972: CIPO 355, 356; and 1974: CIPO 366, 367. United States: 1964: American Institute of Public Opinion (AIPO) 698, 699; 1968: AIPO 770, SRC 1968 National Election Study; 1972: AIPO 858, 859; and 1976: AIPO 959, 961.

Note.—A^* = adjusted index; A = unadjusted index; Canada 1 = CCF/NDP and Liberals classified as left; Canada 2 = CCF/NDP and Social Credit classified as left.

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Commentary and Debate

The comments printed in this section are limited to brief critiques of articles and reviews published in the *AJS*. These comments are expected to address specific errors or flaws. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Authors of articles and reviews are invited to reply to comments, subject to the same length limits. The *AJS* does not publish commenters' rebuttals to authors' replies. Longer or less narrowly focused comments on articles published in the *AJS* should be submitted themselves as articles.

COMMENT ON LENSKI'S "HISTORY AND SOCIAL CHANGE"

In his article (*AJS* 82 [November 1976]: 548–64), Gerhard Lenski criticizes Robert Nisbet's *Social Change and History* (1969) for providing a distorted view of neoevolutionism. Lenski's critique must be faulted on three grounds: (1) his failure to recognize the underlying assumption behind Nisbet's criticism of neoevolutionism, (2) his failure to provide evidence for his own tenuous characterization of the new evolutionists, and (3) his unwarranted claim that the distinction between general and specific evolution should quell most criticisms of the neoevolutionary perspective.

While in *Social Change and History* Nisbet focuses primarily on the "fallacies" of all developmental theories, he also criticizes functional theorizing and what he calls the "conventional wisdom regarding change" in the social sciences. The social sciences are shown to embrace all or most of the central premises of the classical social evolutionary perspective (1969, pp. 235, 270), which he identifies as the following: (1) change is normal, (2) change is directional, (3) change is immanent, (4) change is continuous and cumulative, (5) change is necessary, and (6) change proceeds from uniform causes (1969, pp. 166–82). After identifying these premises, Nisbet proposes six diametrically opposed alternative principles that he regards as more faithful descriptions of empirical reality. Accord-

ingly, the first of the evolutionist premises is discarded in favor of one that states that fixity, not change, is normal and natural (1969, p. 270). The second premise, in turn, is repostulated from "change is directional" to "change lacks directionality" (1969, pp. 284–87), and so on.

Lenski comments disparagingly on Nisbet's "fondness for dichotomies" (p. 553) but fails to discern that Nisbet perceives a common assumption in all developmental theories—the assumption of a compelling determinism. Each of the premises of social evolution detailed by Nisbet reveals this common deterministic framework. For change to be defined as "directional," for example, it is logically imperative that change must be seen as inevitably constrained or made to conform to some natural or destined course of development. Similarly, the proposition that "change is immanent," or proceeds from some internal dynamic, virtually precludes the accidental, the random, the willfully intentional from having significant impact.

For Nisbet, then, the theme of necessity or determinism constitutes the overriding and most objectionable element in neoevolutionism. This is apparent from his criticism of the fourth premise—"change is continuous." He notes that the "real objection . . . is to the fixed notion within the conventional wisdom of social science that one change necessarily engenders another, that one 'stage' of developmental change produces the next stage . . . " (1969, p. 290). The primary shortcoming of neoevolutionism, he tells us repeatedly, is that its concerns with theory, natural history, natural law, and progress leave little room for the multitude of essentially indeterminate developments—"random events," "unique occurrences," "the purely fortuitous" (1969, p. 238), the "willful" and the "adventitious" (1969, p. 171).

Nisbet's commitment, therefore, to an approach in the social sciences that does not preclude on theoretical grounds the importance of indeterminate developments—a more historical approach—accounts for his categorical rejection of neoevolutionism. The question needs to be asked, however, whether the new evolutionism does in fact, as Nisbet claims, subscribe to a fundamental determinism. Lenski would have us believe that Nisbet's depreciation of the new evolutionism is explainable, in part, by his failure to recognize some existing differences between the old and the new evolutionism: "Most [contemporary evolutionists] would not claim that there is necessity in most patterns of change. . . . Most of us would probably be content to say that there are simply varying degrees of probability. One of the important differences between the 19th- and 20th-century forms of evolutionism is the shift from deterministic to probabilistic formulations" (p. 557).

First, who is included in "most of us"? Certainly Sahlins and Service's *Evolution and Culture* (1970), which Lenski cites approvingly, firmly

establishes their perspective as ". . . plain old evolutionary, not neo-evolutionary" (p. 4). And Leslie White, whose concept of evolution as an information-building process is regarded by Lenski as the "single master trend" (Lenski 1976, p. 555) in all of evolution, has this to say: ". . . Neo-evolutionism is a misleading term: it has been used to imply that the theory of evolution today is somehow different from the theory of eighty years ago. We reject any such notion" (1959, p. ix). Indeed, the single reference we are provided with is "Lenski and Lenski 1974, pp. 108–11" (p. 557), in which we encounter solely the authors' argument on behalf of probabilism, followed—without foundation—by the summary statement that "modern evolutionary theory does *not* take a deterministic view . . . " (1974, p. 110).

Second, what is behind the assertion that these unidentified modern evolutionists *"would probably be content* to say that there are *varying degrees* of probability" (italics added)? If there exists no underlying determinism, then why is there an apparent need to compromise—to settle for or "be content" with "varying degrees of probability"? What original position is it that they "would probably" compromise? In summary, the tenuous qualifiers "most of us" and "would probably be content" can hardly be seen as justifying Lenski's sweeping conclusion that a defining characteristic of 20th-century evolutionism is a probabilistic orientation.

But let us grant, for the sake of the argument, that some neoevolutionists have shifted to probabilistic formulations of their theories. What are the theoretical and practical implications of such a shift? For it is only from the nature of these implications that we may judge whether this alleged shift constitutes, as the author claims, an important difference.

It is Lenski and Lenski's *Human Societies* (1974) that provides an insight into the importance of this theoretical shift. "Evolutionary theory," they argue here, "does not assume that technological advance has been inevitable everywhere and all the time. . . . Technostasis and even technological regression have occurred frequently, and evolutionary theory must take account of this" (1974, p. 84). To do so, the authors invoke Sahlins's distinction between general and specific evolution. "General evolution is the evolution of human societies as a whole the world over; specific evolution is the evolution of an individual society. . . . [T]echnostasis and regression are much more relevant for specific evolution, since they have been infrequent phenomena on the global level but fairly common at the level of individual society" (1974, p. 84). As Lenski observes in his article, "Statements that are nonsensical when applied to some instance of the latter [specific evolution] may be entirely valid on the level of general evolution. . . . Many of Nisbet's objections to the newer evolutionism seem to stem from his failure to appreciate the extent to which it has focused on general evolution" (p. 562).

To Lenski the distinction between the two types of evolution enables social evolutionists to address a variety of criticisms. It provides an explanation for such phenomena as the fall of civilizations—phenomena described, significantly enough, as "deviations from the hypothesized pattern of growth and development" (p. 551). It is important, also, that this distinction is used to support the claim that the new evolutionism is probabilistic in its orientation, since it has ceased, according to Lenski, to regard progress in terms of determinism or absolute necessity. It is apparent, however, that Lenski's probabilistic stance remains confined, essentially, to individual case studies, or to the level of specific evolution. In that realm where evolutionists, by the author's admission, have focused their attention—namely, the realm of general evolution—the "shift" away from determinism is far from apparent. In fact, Lenski himself insists that detractors of neoevolutionism must make their case against determinism: "Thus, to disprove the theory that there is long-run directionality . . . critics must show that at the *global level* there is no trend with respect to central tendencies. . . . This, of course, cannot be done . . . " (p. 552). Lenski's assertion, therefore, that neoevolutionists develop their theoretical models in probabilistic rather than deterministic terms cannot be taken at face value. In the area of macro changes social evolutionists consistently point to the existence of some natural or destined course of development, generally expressed in terms of increasing populations, social complexity, and so on.

According to the evolutionist perspective, there are forces that compel sociocultural change to stay on course and sustain the emergence of "superior forms." It is this recognition, one might speculate, that has led White, Service, and Sahlins to insist that their brands of evolutionism are indeed not neoevolutionism, but in the tradition of their 19th-century forerunners—and, therefore, clearly deterministic. Only by defining sustained directionality as probabilistic does one have a probabilistic rather than a deterministic model of change. If Lenski accepts this definition, however, his theory ceases to be what is, in any conventional use of the term, an evolutionary model.

In summary, then, Nisbet's underlying assumption—that the premises of neoevolutionism are grounded in determinism—appears to be fundamentally correct. Contrary to Lenski's assessment, the distinction between general and specific evolution does not make for a more probabilistic evolutionism. The difficulty with Lenski's brand of neoevolutionist position is that, at the primary level of explanation, it remains the evolutionism of the 19th century. Nisbet understands this; Lenski, apparently, fails to do so, and the nature of his attack reflects this failure.

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PROBABILISM REASSERTED: A REPLY TO BECKER

It is not easy to respond to Becker's comments, since he attacks me for a position which I explicitly rejected, not only in my paper "History and Social Change" but also in *Human Societies*, which he cites. If I read Becker correctly, his primary concern is with "the assumption of a compelling determinism," which he, like Nisbet, claims to find in the new evolutionary theory (and in other developmental theories and in functionalism as well). This is the common denominator underlying all three of Becker's criticisms of my paper.

I am glad for this opportunity to respond to Becker, both to set the record straight and to comment on the issue of determinism, which is an important one. For the record, in "History and Social Change" I began my discussion of the question of determinism by saying, "Probably the least objectionable of Nisbet's proposals concerning the nature of change is his assertion that '. . . [T]here is no necessity in any pattern of change if our attention is fixed, not upon the wholes and universals of social evolutionary theoretical interest, but instead upon the social behavior of human beings in time and place.' " In commenting on this, I said, "Actually, most contemporary evolutionists would be even more cautious than Nisbet in one respect. Most would not claim that there is necessity in most patterns of change—*even when attention is fixed on 'the wholes and universals of social evolutionary theoretical interest'* " (italics added). I went on to say, in the phrase Becker chooses to quote, "Most of us would probably be content to say that there are simply varying degrees of probability" (p. 557).

There are two points to be noted here. First, the italicized phrase makes it clear that my commitment to probabilism applies to general evolution, no less than to specific. Second, "the original position" which is being modified (not "compromised," as Becker would have it) is that of none other than Robert Nisbet, who implies a deterministic position regarding general evolution in the statement of his which I quoted. Perhaps Becker

is right that I should have been less conciliatory in my critique of (Nisbet's) determinism at this point.

Lest it seem that these few phrases from my recent paper represent an effort to cover up a more basic commitment to determinism, I would point to two features of the original edition of *Human Societies* (1970), which have been essentially preserved in the second and third editions (1974, 1978). In discussing the rise of the new evolutionary theory in biology, I noted that it involved a shift from deterministic to probabilistic formulations (pp. 59–60) and later suggested that evolutionary theory in sociology should follow this lead (p. 70). Still later, in the closing chapter of the book, I had the following to say in a discussion of the problems of predicting the future in a section headed "Determinism versus Probabilism Once Again": "Earlier in this volume, when we examined the issue raised by deterministic theories, we observed that most social scientists now avoid both extreme positions. They reject the determinists' thesis that the future course of events is inevitable or predetermined, and hence completely predictable if one only has sufficient information on relevant sociological variables. Similarly, they reject the thesis that it is impossible to make any predictions at all. Rather, they take a position that has come to be known as probabilism, maintaining that all *possible* future developments are not equally *probable*" (p. 498). I do not know how I can make my position any clearer.

I must confess, however, that I am troubled when I find intelligent sociologists, such as Nisbet and Becker, repeating again and again the charge that evolutionists are determinists. Why do they do it? And, above all, why do they do it in the face of clear evidence to the contrary and the disavowals of those they attack?

One possibility is that they assume mistakenly that a concern for trends that are sustained for centuries and millennia implies a belief in historical inevitability. Another possibility is that they are humanists who mistakenly, again, assume that any search for regularities in human life, whether by evolutionists or others (and there are clear indications in both Nisbet and Becker that their criticisms are not limited to evolutionists), is a threat to belief in freedom of the will. Or, finally, given their fondness for dichotomies, it is possible that they do not really see any meaningful distinction between probabilism and determinism, since both imply constraints on human freedom.

I cannot help wishing that Becker and others who share his perspective would respond to the assertion of some of us that there are important identifiable patterns in the historical record. What do they make of these patterns? Why do they avoid a discussion of them? Do they deny their existence? If not, how do they explain them? Do they really want us to believe that these long-term trends are merely the result of the fortuitous

and the adventitious, and that somehow or other purely random events fall out in such a way, century after century and millennium after millennium, as to sustain these trends? And, if not that, do they propose that sociology should ignore such trends? Answers to these questions might go a long way toward clarifying some of the confusion which they have introduced.

Before closing, I would like to respond to Becker's question as to who the other recent neoevolutionists are who share my nondeterminist perspective. To begin with, I would never include Leslie White among them, partly because he retired a good number of years ago and is now dead, and partly because his rigid determinism acted as a powerful negative model for me, and probably for others as well. White had some important contributions to make, not least of which was his role as a transitional figure between the older and newer evolutionism, but his position on the issue of determinism has been abandoned, to the best of my knowledge, even by his former students, Service and Sahlins.

More to the point, when I read the work of people such as Boulding (1964, 1970, 1978), Parsons (1966, 1971, 1977), Bellah (1964), or Winch (1978), I find very little evidence of a deterministic perspective. Probabilism, yes; determinism, no. The very use of contingency tables (note the adjective!) by Winch and others of us constitutes an implied commitment to probabilism.

Let me close this statement as I closed my article on Nisbet, with a plea for an end to the quarrel between humanistic historians, with their focus on unique events and unique personalities, and evolutionists, with their focus on the regularities and patterned trends which emerge from the study of the historical record. There is a need for both in today's world. Humanistic history is a valuable antidote to fatalism, just as evolutionism is a check on utopianism. The former reminds us that the human will can influence events, just as the latter reminds us that human will always operates within the framework of external constraints. So let us not subvert the study of the role of the contingent in history by denying its existence, and, similarly, let us not deny the need for the study of historical patterns and trends by refusing to acknowledge their presence.

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COMMENT ON MOLLENKOPF'S REVIEW

"While New York's importance may be declining in some respects . . . , it may actually be increasing in other respects" (p. 475), writes John Mollenkopf in his recent book review (*AJS* 83 [September 1977]: 474–79). If so, we would have a long overdue corrective to the widely held opinion among urbanologists and regional scientists that trends in population and employment are a robust indicator of a region's overall state of economic health. "In fact, declining populations may even be functional" (p. 475). Urbanologists need to be reminded that the "nerve center" of a nation does not have to endlessly accumulate population and capital from the remainder to retain its central role in that nation's economy, and that in fact it need not have very large size at all—the activities in the hinterland that are coordinated by the center may in certain periods call for a disproportionate amount of the nation's energy and physical investment, but the center could remain nonetheless primary for this growth and vigor in the peripheral parts. After all, the brain of a human roughly doubles in size by maturity, while the body reaches 20 times its original size; but the brain retains its central control function despite its slower growth rate. Mollenkopf cites some surprising statistics on trips and messages to and from New York City that seem to demonstrate New York's continued vigor as the nation's nerve center despite its loss of population: "Airline passenger figures into New York have risen steadily, and the Manhattan exchanges are still unexcelled for number of interstate phone calls" (p. 475).

The published statistics on the flow of messages and trips between the various parts of this country that I am familiar with, however, give a rather different impression. Passengers enplaned in the three major airports of the New York metropolitan area have not only not risen steadily (they declined in 1974 and, along with the remainder of the country, during the recession of 1975), but the number enplaned in 1976 is an insignificant 0.2% above the number enplaned in 1973. In contrast, the number enplaned in the country as a whole grew by 10% between 1973 and 1976 (table 1). I know of no published data that give the number of interstate calls processed by the Manhattan exchanges. But the Federal

TABLE 1

TOTAL PASSENGERS ENPLANED

	New York*	(% change)	U.S. Total	(% change)
1973.......	17,639,108	...	202,208,000	...
1976.......	17,674,566	(+.2)	223,313,000	(+10.4)

SOURCE.—Air Transport Association of America, *Air Transport 1974* (Washington, D.C.: Air Transport Association of America, 1974), pp. 3, 34; *Air Transport 1977*, pp. 5, 8.

* La Guardia, John F. Kennedy, and Newark airports.

TABLE 2

NUMBER OF CALLS ORIGINATING FROM TELEPHONES (in Thousands)

	Local	(% Change)	Toll	(% Change)
	NEW YORK STATE			
1970........	18,021,597	...	1,296,834	...
1975........	19,144,800	(6.2)	1,288,688	(−.6)
	U.S. TOTAL			
1970........	169,110,360	...	10,611,420	...
1975........	216,889,110	(28.3)	17,913,387	(68.8)

SOURCE.—Federal Communications Commission, *Statistics of Communications Common Carriers 1970* (Washington, D.C.: Government Printing Office, 1972), p. 16, table 9; *Statistics of Communications Common Carriers 1975*, p. 13, table 8.

Communications Commission does publish data on the number of calls originating in each state on an annual basis. These data show a decline in toll calls originating in New York State between 1970 and 1975 (table 2). In the nation as a whole, toll calls increased by more than two-thirds over this same five-year period. The trends in New York City should track those of New York State.

Until Mollenkopf reveals data that tell us otherwise, students of this nation's cities and regions have no real choice but to stick with their dull but serviceable "one-factor" model of regional economic activity. Not only are population and capital draining off from the center into the periphery, but there is no evidence that higher-level control functions are not as well. (There are a few data points: In 1977, 83 *Fortune* 500 companies have their head offices in the so-called Sunbelt, compared with 59 in 1970 [*Fortune* (June 1977): 144].) Mollenkopf's thesis that the regional hierarchy has remained stable over the 1970s despite radical changes in the relative growth of its different levels is an interesting and imaginative one, but there is little in the data with which I am familiar to make me confident that it has any basis in reality.

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REPLY TO VINING

Daniel Vining has elegantly summarized my argument that the center can continue to coordinate activities in the hinterland even while the latter grows more rapidly. He contends, however, that two indexes which I argue reflect roughly the strength of this coordination role—namely, passengers boarding at New York airports and phone calls made from the city's exchanges—do not support my thesis. There are two issues here: we need to know the actual facts of the matter with regard to plane passengers and phone calls, and we need to specify more accurately what it means for a big city to retain and benefit from a higher-order, coordinating, or controlling role in the urban hierarchy. Although I believe the facts support my original contention, whether they do or not may be irrelevant if the original hypotheses have been improperly framed.

First, the facts. Vining has chosen an aberrant period in which to examine trends, namely, the serious national economic downturn which began in 1972. Since we want to know about New York's business relations, a severe recession is probably not a good time to examine them. And, indeed, if one looks instead at the longer and more representative period between 1965 and 1976, one finds that New York's passengers have increased 70% while the nation's grew 147%. More important, if one examines the networks among cities, they remain reasonably stable with New York at the center. This pattern changes mainly as cities which appear themselves "tributary" to New York extend their own penetration into smaller cities.

Phone calls are more complicated. New York State figures contain many stray influences other than New York City's business influence. (Only about half the state's phones are in the city.) And out-of-state phone calls, even more than plane boardings, reflect a broader set of motives than business-related activities alone. Finally, leased lines and computer-driven communications have totally revolutionized the meaning of a phone call over the past decade. Even so, a spokesman for the AT & T long-lines division assured me off the record that their New York City equipment is being constantly upgraded to respond to increasing demand. Interestingly, AT & T declines to publish data on point-to-point communications patterns for fear of alerting competition to potential markets. This suggests that a look at what AT & T's competitors are trying to do would reveal New York's importance. And, indeed, the first competing transmission system is a new tie between New York and Houston. While this suggests Houston's growing importance, it certainly does not suggest that New York is on the decline. In short, to the extent that we can make inferences from longer-term trends in these two admittedly crude indicators, they

support rather than "flatly contradict" the argument as initially stated.

This leads us to the second issue: Aggregate statistics about individual cities and their relative rates of growth and decline are to some degree beside the point, because they do not measure the interactions between cities very well. The essence of the "retention of control" hypothesis is that actors located in New York play important roles in things that happen in other cities, and, conversely, that actors in the other cities cannot decide their affairs among themselves alone but must somehow involve New York actors. Direct evidence on such exchanges across cities (whether they take place in the public or the private sector) would be vastly superior to indirect indicators such as New York's aggregate volume of telephone calls. Unfortunately, few data of this sort are published. If any reader has detailed information of this type, whether for phone networks, bank clearings, or whatever, all parties would undoubtedly be glad to hear about it.

Such crude information as is available also supports the retained-influence hypothesis for New York. Although *Fortune* 500 firms have moved away from Manhattan, and although the number located in the Sunbelt has increased, the New York metropolitan area has actually retained the same number of headquarters over the past decade or so. And this fact should not be particularly surprising, given that New York has retained its role at the top of the hierarchy through many periods of substantial social change. (For an interesting study of the stability of the urban hierarchy, see Madden [1955/56].)

To conclude, the empirical indicators challenged by Vining support my case, as does other admittedly partial and indirect evidence. Yet he has done a service by pointing out that we need to have a much more specific investigation of the question, both in terms of how the relevant hypotheses are framed and what type of evidence is gathered. A definitive resolution clearly awaits further research.

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Review Essay: On Tourism and Modern Culture[1]

The Tourist: A New Theory of the Leisure Class. By Dean MacCannell. New York: Schocken Books, 1976. Pp. xi+214. $12.95 (cloth); $5.95 (paper).

Image and Pilgrimage in Christian Culture: Anthropological Perspectives. By Victor Turner and Edith Turner. New York: Columbia University Press, 1978. Pp. xvii+281. $15.00.

Hosts and Guests: The Anthropology of Tourism. Edited by Valene L. Smith. Philadelphia: University of Pennsylvania Press, 1977. Pp. 254. $14.00.

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Freud constructed psychoanalytic theory out of attention to the trivial occurrences of everyday life, such as dreams and slips of the tongue. Dean MacCannell tries to reconstruct social theory by attending to a commonplace escape from everyday life—tourism.

MacCannell would not be surprised at the comparison with Freud. As he undertakes his ethnography of tourism, he argues that ethnography "will eventually occupy a position in the modern world similar to the one occupied by psychoanalysis in the industrial world" (p. 174). The modern or postindustrial world, in MacCannell's view, is the "coming to consciousness of industrial society" (p. 182). It is marked, in the world of science, by sociology, and in the everyday world by tourism. MacCannell argues that the tourists frequently surpass the sociologists in understanding the meanings of modern life. And well they might, for, if MacCannell is right, tourist attractions provide the language through which people in modern society seek an authentic understanding of their world.

This notion may sound a little loony. *The Tourist* has an unflattering habit of announcing revelations rather than sharing information. It is pockmarked with ex cathedra pronouncements ("the courtroom is the most important institution in a democratic society") and epigrammatic misrepresentations of social theory ("Marx and Mao are committed to the priority of the material substratum over theory and ideas"). Still, it is a serious stab at, all at once, creating useful categories for the study of tourism and other forms of leisure; exploring the nature of authenticity in modern life; and seeking to construct grounds for an ethnography of complex society, a way

[1] I want to thank John MacAloon, Carl Pletsch, Mike Ryan, and Robert Snow for stimulating my thinking on tourism.

of comprehending the signs by which a highly differentiated society is given meaning. The book is not what the subtitle says it is, a "new theory of the leisure class." One of its more egregious faults is that it says nothing about class at all. MacCannell is "suspicious of research that insists on the primacy and independence of social class" (p. 11). This is scant justification for ignoring class entirely. But if *The Tourist* is not a theory of the leisure class, it *is* a theory of leisure. And it is, warts and all, a dazzling book.

There are good grounds for sociological attention to tourism. Take simply the place of tourism in the world economy. In 1950, international tourism alone was a $2 billion business. In 1960, it accounted for $6.8 billion; 1970, $17.9 billion; 1975, $31.9 billion. The number of international tourists has grown enormously. In the United Kingdom, for example, there were just over 600,000 tourists in 1950, 1.7 million in 1960, and 5.9 million in 1970. Overall, there were 72 million international tourists in 1961 and 168 million in 1970. In 1965, international tourist receipts represented 3.4% of total exports for the United States, 14.3% for Greece, 19.0% for Ireland, 39.4% for Mexico, and 47.7% for Spain. While Europe and North America still attract the most tourists, Third World countries have actively promoted tourism, too, with the United Nations, the World Bank, and other aid agencies assisting them.[2]

That tourism is of great and growing economic significance has made it politically important. Governments, both national and local, are involved in promoting and in regulating tourism. Whereas the creation of tourist attractions (like Disney World) has been the work of entrepreneurs and developers, the preservation of sights of possible touristic interest has concerned a wide variety of individuals and groups, including community organizations, universities, banks, and city-planning commissions. Historical preservation has become a major consideration in the past several decades and frequently takes the shape of a social movement. Neighborhoods not only restore themselves physically by an appeal to tourism, but also reawaken culturally. Whole regions and whole countries may do so, too.

All this, however, while clearly of concern for the sociology of economic development, urban sociology, and the study of social movements, is not what MacCannell proposes as the central sociological question about tourism. He believes that tourism provides the symbols by which modern people give their lives meaning. Ours is not a world of religion or even of civil religion, in his perspective, but of tourism. The travel brochure, not the religious tract, is a source of inspiration. While, as I shall indicate, MacCannell takes this too far, I think he is right to suggest that a sociology of culture, broadly conceived, should pay attention to tourism, that the roadside historical markers we pass by or, more rarely, stop at, are signs of how we conceive our society and ourselves.

There is little in the classical tradition of either sociology or anthropology to help us study tourism. One thinks, perhaps, of the literature on "marginal men." But the marginal man stands outside or next to the mainstream

[2] For statistics on tourism, see Gray (1970), Popovic (1972), British Tourist Authority (1971), and World Bank (1972).

of society; the tourist stands above it. The stranger lives as a stranger; the tourist *looks* as a stranger but lives somewhere else.

One may turn to the sociology of religion for guidance. There is a difference, of course, between a "holy day" and a "holiday," between a vacation "taken" or "spent" and a holy day "observed" or "celebrated." But MacCannell argues that we must understand tourism as religious ritual. According to him, the tourist is the model for "modern-man-in-general," and tourism is one of the two poles of modern consciousness. The other pole, revolution, is the desire to transform things; tourism is the inclination to venerate things as they are. Tourism is a ritual activity. Tourist attractions are "precisely analogous to the religious symbolism of primitive peoples," and sightseeing can be defined as "a ritual performed to the differentiation of society." Modernity, for MacCannell, *is* structural differentiation, the source of both the sense of freedom in modern life and the sense of alienation, fragmentation, and discontinuity. Tourism explores, venerates, studies, and shares in structural differentiation.

That is MacCannell's—I think largely mistaken—claim. Just what does it mean? Fortunately, MacCannell is forthcoming with illustrations drawn from tour books and other travel literature and from his own field notes as a participant-observer tourist. The object of tourism is society itself. Tourism tries to see society and grasp its connections in an enjoyable way. Workers, alienated from their labor in the factory, may nonetheless tour some other place of work, the FBI or the mint or another factory, and enjoy watching the work that they would not enjoy doing. Tourist "sights" may include all the different features of society. One may tour neighborhoods, work districts (the garment district in New York), domestic establishments ("Society Hill" in Philadelphia or antebellum mansions in Natchez), groups (Pennsylvania Dutch or Indians on reservations), occupations (blue-collar work in a factory, journalism in a newsroom, farming at a dairy), transportation (the Golden Gate Bridge), and public works (MacCannell cites a tour of London's public lavatories). MacCannell discusses turn-of-the-century guidebooks to Paris, including Baedeker's, which he calls "still the best ethnography of modern cities." The Paris guidebooks direct the visitor to and give detailed descriptions of the stock exchange, the morgue, the supreme court, the mint, the government printing office, the Gobelin tapestry weavers, a tobacco factory, a slaughterhouse, and the sewers. In the tourist's guidebook, experience is democratized. Everything deserves attention. Just as, in Durkheim's view, primitive peoples unknowingly worshiped the power of society itself in their totems, so, in MacCannell's perspective, tourists sacralize the variety of social formations, the sum of which represents the forces of society.

For MacCannell, however, not all tourist attractions are equivalent. They differ according to their degree of authenticity. Religion has to do with being in the presence of ultimate reality and feeling subject to the power of the ultimate. Tourism, in its own fashion, is a quest for the ultimately real or, as MacCannell puts it, for the authentic in human experience. MacCannell orders tourist settings along a continuum of authenticity. Some attrac-

tions are purely "front," set up solely to attract tourists. Others are purely "back," not designed for tourists at all but come upon only by chance or persistence—these are "unmarked" attractions. In between, there are fronts organized to look like backs (a pioneer village like Old Sturbridge), backs open to tourists (factory tours), or backs spruced up a bit for visitors (the open orchestra rehearsal). The whole range of attractions plays on the modern passion for experiencing reality and participating in the authentic. In a marvelous aperçu which dramatizes this argument, MacCannell writes that the person who wants to get off the beaten path, who disdains commercialization and the most superficial tourist shrines, is not the antitourist but the supreme tourist: ". . . touristic shame is not based on being a tourist but on not being tourist enough, on a failure to see everything the way it 'ought' to be seen. The touristic critique of tourism is based on a desire to go beyond the other 'mere' tourists to a more profound appreciation of society and culture, and it is by no means limited to intellectual statements. All tourists desire this deeper involvement with society and culture to some degree; it is a basic component of their motivation to travel" (p. 10). Seeking the authentic, which means avoiding commercialization, avoiding other tourists, and even avoiding consciousness of oneself as a tourist, is the essence of tourism.

That is MacCannell's argument, always provocative, sometimes brilliant, and, I think, wrong. Is it true that tourists seek authenticity? Some do, some do not, and the tourist industry caters to both kinds. Large numbers of tourists, even when traveling abroad, seek food they recognize, music they are familiar with, and comforts to which they are accustomed. H. Peter Gray distinguishes two forms of tourism—"wanderlust," whose aim is education, and "sunlust," whose aim is rest (Gray 1970, p. 14). This suggests correctly that, contrary to MacCannell, tourism is not a system of attractions. Instead, it is a system of attractions and *dis*tractions. Tourism is as much, or more, a moving away as a moving to. It is escape as often as involvement.

In this sense, tourism is not "precisely analogous to the religious symbolism of primitive peoples." MacCannell himself observes that religious shrines in modern society lose their traditional value and take on touristic significance: "Throughout the world, churches, cathedrals, mosques, and temples are being converted from religious to touristic functions" (p. 43). But this admits that modern ritual (which is not religious) differs from primitive ritual (which is). MacCannell's assertion that tourism is ritual is too glib. So is his view that tourism has moral significance—after all, he asks, do we not speak of a tourist attraction as a "must see"? But if you pass up a "must see," you do not feel you have transgressed divine or social codes. You have simply failed to do right by yourself, failed to experience something you might have taken in. MacCannell makes a contribution in urging us to attend to the ways in which tourism is a crusade for authenticity, but he is misleading in suggesting that this is all of tourism or even the essence of tourism. What may be more essentially touristic is not a search for the authentic but a doubt that it can be found. One feels more

sure that a touristic journey will provide distraction than meaningful attraction, and tourism lives in the space between those poles.

Even this, however, gives MacCannell's case too much credit, for it assumes that we are all modern and, as moderns, engage in tourism. But this is not so. In any given year, in the United States, most people are tourists, if at all, only in their own communities. In 1972, 45% of Americans (or 37% of American households) did not take a single trip to a destination 100 miles or more away from home. Among those who did travel, 39% of their trips were to visit friends and relatives, 20% were for business and conventions. Freud seems to have been right—love and work are what matter or, at least, what still move people the most. Only 12% of trips were for outdoor recreation and 13% for sightseeing and entertainment (U.S. Bureau of the Census 1973).[3] Of course, these data are not conclusive, but they at least suggest that people organize the meanings of their lives in a circle centered on home and family, work and community, rather than in a far-flung system of tourist shrines.

When tourism does seem most surely a quest for authenticity, is it in fact a ritual "performed to the differentiation of society"? I think not. On the contrary, when tourism is most nearly religious ritual, it is an escape from social differentiation, an effort to transcend the limits of daily social experience by coming into contact with nature, or with culture in the form of unifying symbols and myths which transcend social differentiation. MacCannell acknowledges that attractions at the heart of tourism may escape commercialization, and here he refers to natural sights. The Grand Canyon, despite tours, hotels, postcards, and the Chamber of Commerce, is not reproducible. It can be experienced in only one place in the world. MacCannell finds it significant that California, a state rich in natural wonders, should be forty-seventh in the amount of state funds spent to promote tourism. Why is there such a persistent popularity of natural sights if tourism is a worship of social differentiation?

On the other side, if tourism worships differentiation, what accounts for the popularity of sights of cultural unity? Why is tourism the second largest industry in Washington, D.C.? Why do millions visit Lenin's tomb every year?

There is, I think, an answer to these questions. It is implicit in a fascinating new book by Victor Turner and Edith Turner, *Image and Pilgrimage in Christian Culture*. The Turners discuss the symbols and social structure of Christian pilgrimage, covering both medieval pilgrimages and, in greater detail, contemporary pilgrimages in Mexico, Ireland, and France. Their main interest is "the theological doctrines and popular notions which promote and sustain Christian pilgrimage, and the symbols and images which embody them." Much of the book, then, a rather fine-grained anthropological account of particular symbolic structures, is not related to the concerns of this review, but some of it can be read as part of a conversation with students of tourism as a social and cultural phenomenon. The Turners

[3] In addition, 16% traveled for "other" reasons.

can be seen to offer both significant support and serious critique of the kind of position MacCannell takes.

The support is simply this: the Turners suggest that tourism is worth serious attention from social scientists. In their chapter on medieval pilgrimage, they indicate that the serious meaning of a pilgrimage was often covered over by the barnacles of a tourist trade. Pilgrim ways in the Middle Ages were lined with way stations that displayed sensational relics, many of which, like the trinkets of tourism in a later age, assumed the values originally attached to the place to which the pilgrimage was made. The relics became fetishes "operating by principles of sympathetic and contagious magic, rather than serving as vehicles of religious and ethical ideas" (p. 197). Bordellos were easily available along pilgrimage routes. Adultery was rumored to be common at pilgrim shrines. Pilgrimage tended to lose its penitential quality and to become "worldly and fashionable." Reformers and satirists attacked pilgrimage and "began to raise a cry which became louder and louder until it was something like a storm at the time of the Reformation" (p. 184).

Despite the corruptions of pilgrimage, it played (and still plays today) a vital cultural role in two respects. First, it creates what the Turners call a socioeconomic "field." In the Middle Ages, pilgrimages stimulated and shaped the growth of cities, marketing systems, and roads—perhaps as much, the Turners suggest, as purely economic and political considerations.

Second, pilgrimage did, for many, and still does today, provide people with a sense of relationship to ultimate values. Here, I think, is the kernel of a critique of MacCannell. The Turners suggest that pilgrimage, far from being a worship of society, is a transcendence of the social, an experience that, drawing from the language of Victor Turner's earlier works, lifts people out of *societas* or "structure" and into *communitas* or "antistructure." In new terminology borrowed from Mihaly Csikszentmihalyi's (1975) studies on play and games, the Turners argue that the meaning of pilgrimage lies in the experience of "flow" it engenders. Flow is "the merging of action and awareness, the crucial component of enjoyment." Flow, experienced in play, sport, artistic performance, and religious ritual, is the product of centering attention on a limited field, assisted often by a set of rules. We then act with total involvement, aware but not aware of our awareness: "There is a loss of ego, the self becomes irrelevant." The Turners suggest that pilgrimage has survived the centuries and, in one form or another, may be regarded as a cultural universal, because it has regularly succeeded in producing this overwhelming feeling of enjoyment, "flow."

Is there a sense of "flow" in tourism? When tourism is most like pilgrimage, when it is "some form of deliberate travel to a far place intimately associated with the deepest, most cherished, axiomatic values of the traveler," there may well be a sense of flow. Pilgrimage, plain and simple, is still a major element of modern travel, as the Turners show. Tourist bureaus and travel agencies must be well aware that 700,000 people annually travel to Knock in Ireland, 3 million a year go to Lourdes, 3 million to the cathedral of Santiago de Compostela in Spain, and more than 5 million to the shrine

of Guadalupe in Mexico. Secular travel which is most like pilgrimage, I would suggest, is that which draws people out of social diversity into nature or into symbols of national or international unity. Contrary to MacCannell, the authentic attractions people seek are not things that are genuinely what they are but things that are *more* than what they are. The Lincoln Memorial is accordingly richer than the mint or the FBI. Indeed, the machine or tractor displayed at the world's fair, where it represents national power and solidarity, has a richness of association greater than the same machine or tractor displayed to tourists in its more "authentic" setting on factory or farm.[4]

MacCannell's book can be contrasted to a recent collection of papers by anthropologists, *Hosts and Guests: The Anthropology of Tourism,* edited by Valene Smith. *Hosts and Guests* is a very different, and lesser, effort than MacCannell's at understanding tourism. Smith et al. look at the effects of tourism on small-scale societies, while MacCannell attends to the meaning of tourism in modern societies. Smith et al. concentrate on the people who become tourist attractions or workers in the tourist trade, while MacCannell focuses on the people through whom they make their living, middle-class tourists. MacCannell's is a book of great pretension, Smith's one of modesty. It includes short pieces on Eskimo tourism and on the manufacture of *molas* by the San Blas Cuna Indians of Panama. In his contribution on tourism in Tonga, Charles F. Urbanowicz tells us that the Tongan Visitors Bureau found tourists disappointed that Tongans knew little of their own culture and history, so the bureau published a pamphlet for local consumption to improve on this score. A piece on Bali suggests that tourism may "strengthen the process of conserving, reforming, and recreating certain traditions." On the other hand, Davydd Greenwood documents how tourism changed the *Alarde,* a major public ritual in the Spanish Basque country, from a vital commemoration of a victory over the French in 1638 to an obligation to be avoided. The municipal government urged that the ritual be given twice in the same day, redefining it as a "public show to be performed for outsiders." One of the best pieces in the volume is Oriol Pi-Sunyer's discussion of native-tourist interaction in Catalonia, an essay which neatly undercuts the volume's "hosts and guests" title. For, as Pi-Sunyer argues, the native-tourist relationship is the opposite of the traditional host-guest relationship of Catalonians: "If tourism commoditicizes culture, natives categorize strangers as a resource or a nuisance rather than as people" (p. 155).

Unfortunately, the essays are afterthoughts to anthropological fieldwork, not genuine case studies. In few of them do we hear native voices. This is not the "anthropology of tourism" it hopes to be—actually, MacCannell's study of guidebooks comes closer to that, closer to investigating tourism itself as a cultural system. Of course, the anthropologists have no more resources for dealing with tourism in their tradition than do the sociologists.

[4] Attractions of cultural unity are sometimes less compelling than people most eager to stimulate cultural unity would like. Philadelphia anticipated 14–45 million visitors during the bicentennial and got 7–10 million instead. As Michael Zuckerman laments, "Surveys showed that travelers were celebrating the bicentennial in California, the Southwest, and Hawaii instead" (Zuckerman 1978, p. 225).

As Smith observes, anthropologists have been slow to address themselves to tourism. The first published article was in 1963; the first national symposium, which generated *Hosts and Guests,* was in 1974.

One result is a failure to find a consistent tone or stance regarding tourism. Some of the essays try to be analytic, some are oriented to questions of policy ("Tonga dances have a beauty and symmetry of their own that should be performed for the visitors to Tonga"), and some sound like cliché-ridden copy for the tourist industry itself ("North Carolina is rich in natural resources, and her coastal area is unrivaled for sheer beauty anywhere on this continent. Her greatest wealth, however, lies in the people of North Carolina"). A question the anthropologists are all concerned with is, Is tourism good or bad for local people in developing parts of the world?

They awkwardly avoid an answer. The tone of the book conveys a rather studied refusal to be judgmental. The authors sound as if they are trying to convince themselves that tourism is potentially a good thing. Theron Nuñez, in the concluding essay, notes the tendency of anthropologists to be hostile to the very notion of tourism. While urging anthropologists to overcome this attitude, Nuñez reveals his own allegiance to it: "At the present state of our knowledge it is difficult to demonstrate that tourism *per se* is uniquely destructive or evil." Smith similarly fights her instincts in order to say something nice about tourism. While noting unhappily that tourism leads to the transfer of local control to national governments and the domination of local trade by international business, she argues still that "the tourist trade does not have to be culturally damaging." She believes that a little tourism is a good thing but that "mass tourism" is likely to dehumanize both hosts and guests. Indeed, she distinguishes several types of tourism according to the number of tourists and their degree of adaptation to local norms. The greater the number of tourists (as in "mass" tourism or "charter" tourism), the poorer the adaptation; the smaller the number of tourists ("explorer" tourism or "elite" tourism), the better the adaptation.[5]

This stance is exactly what MacCannell, in his final chapter, calls the "antitourist" position. Antitourism, he suggests, is a position of modern urban liberals and Third World radicals which "can be reduced to a negative response to the recent expansion of the tourist class to incorporate larger numbers of different kinds of people" (p. 164). Antitourists enjoy their own tourism but oppose "these other tourists." MacCannell's own position is that both protourists, who favor tourism for economic development (despite evidence that tourism can help "underdevelop" a nation by keeping capital in foreign hands and providing primarily menial employment), and antitourists are responding to the growth of tourism which has outstripped "support" institutions. He sees a solution in increasing "the number of marked attractions and support facilities for tourists on a worldwide base." But, he acknowledges, this may not work. To skip industrialization and

[5] There is surely a need for more attention to the social costs, and benefits, of tourism. For other comment on the question, see Bryden (1973, pp. 91–96) and Wood (1979); see also Wood's unpublished paper, "International Tourism and Cultural Change in Southeast Asia."

jump headfirst into modernity through becoming a tourist "attraction" is a dangerous path for a nation. "If a group avoids industrialization as it modernizes, it may end up spending all of its time wondering if it is real or not—that seems to be the price paid for rapid advancement in the modern world."

That may also be the price paid for a book, like MacCannell's, which playfully leapfrogs over Marx and Veblen, pokes at Lévi-Strauss and Goffman and Benjamin (while taking some hints from them), and absurdly claims to have transformed the study of modern culture from "theoretical speculation" to "empirical studies" (p. 110). Foolhardy it is to claim to be doing an ethnography of tourism without a word about the age, sex, class, nationality, or personality of tourists, without a word on the daily life of tourism, without mention of the social organization of the support institutions of hotels, airlines, and tour companies. (And, of course, they are not only support institutions, but institutions which invent new attractions to support themselves.) MacCannell does not come to grips with the problem posed by the importance of natural sights, nor does he even acknowledge the problem posed by sights of cultural integration for his theory. All this leads the reader to wonder if *The Tourist* is real or not. It has, perhaps, advanced too rapidly, from unquestionable insights to theoretical parody.

One early tourist MacCannell quotes as having written in 1876 (Arthur Young) in fact wrote in 1790, a time when the usual tourist was still the aristocrat on the grand tour, an eighteen-year-old equipped with guidebooks, a pistol, and a telescope and often accompanied by a tutor. Even at that time tourists could be nuisances. Voltaire, at his estate at Ferney, found himself too often a tourist attraction, and once snapped at visitors who, unwelcomed, were staring at him: "Well, gentlemen, you now see me. Did you take me for a wild beast or monster that was fit only to be stared at as a show?" (Hibbert 1969). It is not a fair question. If tourists keep their distance from some attractions, it may be as much from humility as from arrogance. It is a ritual distance, if MacCannell is right. It is an "observant" stare. What this does to celebrities, who become sights, is worth considering in the same breath with what it does to the people who live in tourist neighborhoods or to the people of the Bahamas, where nearly two-thirds of employment is in the tourist trade.

If Daniel Bell is right that culture has come unhinged from social structure, if culture now leads rather than follows social change, so consciousness, as MacCannell suggests, may not be an outgrowth of structure so much as the definition of what social structure is today. Alternatively, if the Turners are right, then culture has long been a template for social change. People's need to participate in a meaningful cosmos has had quite as much to do with the patterns of settlement and commerce as have more strictly economic needs. The economy of the soul as well as the economy of the body must be satisfied. Indeed, the Turners speculate, the seeds of new social structures may be sown in old antistructures, and medieval pilgrimage may have been "a prototype, under different socioeconomic conditions, of more egalitarian, voluntaristic, contractual types of secular social relationships."

In any case, the problem MacCannell started with is a serious one: Is an ethnography of modern, differentiated, and worldwide integrated society possible? Is there any way to capture the meaning of modern experience? At least, as MacCannell begins to show, and as the Turners show in focusing on contemporary mass phenomena more closely linked to traditional religion, we can examine the ways in which we and others seek for meaning. We can examine the experiences of pilgrimage and tourism, the ways in which people gather meanings at their shrines, and capture them for future reference in memory, in photographs and films, or, for that matter, in monographs.

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Review Essay: Feminism and Sociology–Some Recent Perspectives

Dependence and Exploitation in Work and Marriage. Edited by Diana Leonard Barker and Sheila Allen. London: Longman, 1976. Pp. viii+265. $15.50 (cloth); $7.95 (paper).

Sexual Divisions and Society: Process and Change. Edited by Diana Leonard Barker and Sheila Allen. London: Tavistock Publications, 1976. Pp. ix+287. £6.50 (cloth); £3.50 (paper).

Being Female: Reproduction, Power and Change. Edited by Dana Raphael. Chicago: Aldine Publishing Co., 1975. Pp. xiii+293. $14.50.

Woman, Culture and Society. Edited by Michelle Zimbalist Rosaldo and Louise Lamphere. Stanford, Calif.: Stanford University Press, 1974. Pp. xi+352. $12.50 (cloth); $4.75 (paper).

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Sociological capital can be made out of social protest movements. Specific sociological products (books, articles, doctoral dissertations) are, of course, only one representation of the social resonance of protest. Moreover, sociological analysis is bound to annex protest to its own mainstream concerns; the task of social science, as many of its practitioners see it, is the intellectual colonization of social action. Yet certain brands of protest may be assimilated into prevailing modes of sociological (or anthropological) analysis only with great difficulty.

The relationship between sociology and feminism is a complicated one. Within the feminist movement sociologists are very often mistrusted because they are seen to turn women (like other social actors) into objects of study, whereas feminism decrees that women cease to be objects and become, properly, subjects of their own experiences. Marxist feminists further complain that sociologists implicitly support the exploitation of women because they are themselves agents of capitalist ideology (even if their work is not directly financed by a government or geared specifically to the indoctrination of future workers). Yet there exist, paradoxically, feminist sociologists—as the four volumes reviewed here evidence. This conjunction implies that sociology is not feminist; if this is so, feminists might reasonably inquire, what business do feminists have with it? Such an inquiry is double edged. In the first place, one wants to know in what ways sociology runs counter to feminist concerns. Second, the concern of feminism must be with the usefulness of sociology as one among many de-

vices for developing a concrete understanding of women's situation (historically, contemporarily, and cross culturally).

Each of these volumes is a collection of essays on different dimensions of being female and on the disjunctions and contradictions created by the attempt to marry conventional social science with feminism. There are five editors and 70 contributors; the subject matter of the collections, all published between 1974 and 1976, can be taken as one index of the manner in which social science, since the mid-1960s, has responded to the challenge of the women's movement. To list the subjects covered provides a rough guide to the focus of response. The largest number of pieces concerns motherhood and aspects of gender role differentiation (11 essays each); marriage is next (nine essays); followed by women's social networks, paid work, and domestic power (five each); then socialization (four); nondomestic politics, law, and the position of women in general (three each); and housework (two). The agenda thus implicitly disputes the importance, at this particular moment in our understanding of women's position, of looking at women's participation in formal institutional structures—politics, organizations, religion, and so on—and instead highlights the central significance of the domestic domain, informal structures, and general patterns of social division between the sexes in influencing the status of women. There is a concomitant stress on ideology—its meaning, creation, maintenance, and impact—at the expense of a materialist approach to the role of women in different cultural milieus.

Perhaps the most striking development in sociological literature on women reflected in these works is hidden by the colorful rhetoric of particular concerns. The onus is no longer (as it was a decade ago) on the feminist social scientist to prove the subordination of women in Western industrial society; this has become a taken-for-granted fact. Modern social institutions are guilty until proved otherwise, and it is up to the defenders of equality to identify nonsexist practices, beliefs, causes, and consequences as evidence against the (now implicit) allegation of patriarchy. (The word "oppression" is still, however, not much favored by social scientists, particularly in Britain; the American authors in these collections are more outspoken about their ideological commitment.)

Everyone in these volumes is agreed that in modern industrial society, at least (or especially?), women are regarded and treated as inferior, and that within the dominant ideology feminine concerns are trivialized and masculine ones inflated, so that a girl learns the devaluation of femaleness from the cradle to the grave. The most interesting pieces are those which combine original research with a critical attitude toward mainstream sociology's (or anthropology's) definition of woman's place. In their equitable preface to *Sexual Divisions and Society,* Diana Barker and Sheila Allen quote the following lines from Vanessa Maher's study of relationships among Moroccan women (prefaces in such collections are often the most fertile hunting ground for good ideas): " 'Only after a substantial, polemical, and varied experience of the society in question is it possible to dis-

cover that statements gravely and precisely articulated are statements of ideology, reflecting either the behaviour of the dominant social group, which may be the minority, or the ideological tendency to confuse the substance of an act with its legal enactment, thus attributing the agency of all significant social processes to men,' " (p. 3). This applies both to the common procedure among anthropologists of acquiring information from elderly, high-status males and to much of what has traditionally passed for social science. In her extremely thoughtful "Theoretical Overview" in *Woman, Culture and Society,* Michelle Rosaldo points out that much traditional ethnography, in describing the symbolic conceptions of woman's place that prevail within dominant masculine ideology, has ignored the fact that it is just these conceptions that may be used by women as a basis for female solidarity. Convents, brothels, informal neighborhood friendships, women's political organizations (in Africa, for example) are some instances of women's cooperation that coexist with an ideology that sets women apart and appears to constrain their activities. The culture of women is not necessarily identical with women's culture as portrayed by men, and, as certain of these authors make plain, special techniques are required by ethnographers from Western or other dominant cultures who are concerned with exposing the culture of a minority group. A logical consequence, which is barely touched on in any of these volumes, is that the analysis of women's culture as in some sense a counterculture to that of men implies the same masculine viewpoint that feminist social science is now challenging. So, for example, it is theoretically possible that rather than masculine conceptions of women providing a symbolic basis for women's consciousness and community, the existence of female solidarity precedes the erection by men of a symbolic account which allows them to feel they are controlling the place of women in society.

In what I suspect is, at times, an unthinking way, some of these pieces borrow the language of masculine institutions to describe female domains. Thus Ronald Frankenberg, in an amusing account of sexism in his own and other people's work on community, refers to "the industrial sociology of the housewife" (*Sexual Divisions and Society,* p. 28), and Ann Whitehead, in a pertinent analysis of female-male relationships in a Herefordshire village, talks of "the industrial sociology of child-rearing" (*Dependence and Exploitation,* p. 198). The use of such language certainly reflects the paucity of conceptual development as applied to the domestic world (and for this reason perhaps should be excused), but the danger is that, as with certain models of sex equality or liberation, we are simply fitting women to the contours of the masculine stereotype. This obfuscates the representation of a female perspective and in the long run is not the appropriate medicine with which to cure the inherited sexism of social science. As Rosaldo notes in her "Theoretical Overview," there are two arrangements which can potentially elevate women's status: either women may enter the public world (of men) or men may enter the domestic world (of women). But the formula needs a caveat, for societies tackling gender inequalities

have learned over the past 10 years that it is not enough to increase the participation of women outside the home. Individual women who benefit from expanded opportunities in the public domain may enjoy higher status, but the position of those who do not is entirely unaffected. The low status of domestic work and child rearing, caused by a continuing connection with devalued femininity, persists and is even intensified by the creation of two "classes" of women—those who can pretend to be like men and those who cannot.

The papers included in the two volumes edited by Barker and Allen were all given at the 1974 British Sociological Association conference entitled "Sexual Divisions and Society." This was the first formal sociological occasion in Britain which recognized gender roles as a serious object of study, one which was no longer to be swept under the carpet or dealt with in the odd paper or the occasional paragraph or two as "the problem of women." In this respect British feminist sociologists are slower on the uptake and less self-assertive than their American counterparts; Barker and Allen relegate to a footnote (*Sexual Divisions*, p. 18) their comments on the professional sociological (male) reaction to the idea of a conference on gender roles. I would have liked to see in print a detailed account of this historic occasion—an account which included all the backroom scenes (for example, the pressure from feminists outside sociology to choose a title more specifically focused on the position of women than the gentle and unexceptionable "Sexual Divisions").

One important task of feminist social scientists in our culture seems to me that of offering a continuing ethnography of the development of gender relations. To focus one's analysis on the situation of women rather than that of men (because women are the minority group) does not and should not preclude attention to the relational aspect of gender. A major theme emerging from these collections is the crucial impact of the context in which gender behavior is studied. Colin Bell and Howard Newby (*Dependence and Exploitation*, pp. 152–68) put the argument that women's deference is contextual. It is in the "dialectic" of marriage, where women feel themselves to be, are, and are expected to be relatively powerless in relation to men that feminine deference is most prominently displayed and most legitimated as a form of social (gender role) relationship. Much the same point is made by Marilyn Hoskins in an essay on the roles and options of Vietnamese women (*Being Female*, pp. 231–48). She draws a contrast between the formal respectful language and deferential attitude displayed by Vietnamese wives to their husbands and the role of many women in running the family business and handling the family income; both behavioral complexes are part of cultural womanhood in Vietnam.

Sensitivity to situational dimensions of gender role behavior follows from the growing input of research in this area. So, also, does a questioning of conventional research categories: marriage, work, socialization, politics, authority, and so forth. Increasingly it seems that the opposition between domestic and public domains in most, if not all, cultures is the main struc-

tural component of gender differentiation. Materially, socially, symbolically, and ideologically men and women are separated through their differential commitments in and outside the home. To assert this—as several contributors to these volumes do—clearly requires greater specificity about the meaning of "domestic." Just what is it that women are identified with, and—a different but essential question—why is it so?

In a fascinating chapter entitled "Family Structure and Feminine Personality" (*Woman, Culture and Society,* pp. 43–66), Nancy Chodorow puts forward the old argument that women's mothering role accounts for their universal subordination. However, Chodorow's proposal has none of the debased biologism of traditional polemics. Briefly, her contention is that because women give birth to children they are likely to play a larger part in their rearing than men; through early socialization by the mother, both female and male children experience the beginning of gender personality differentiation even before the onset of separate gender identities. (Thus Chodorow opposes the traditional psychoanalytic model in maintaining that the pre-oedipal experience is already different for boys and girls.) Feminine gender identity, on the one hand, is personal and consists of a long, unbroken, and, in some ways, unbreakable identification with the mother. This type of identification does not necessarily lead to personal adjustment since, Chodorow argues, the failure to separate from the mother is the source of many problems encountered by Western women (for instance, the fact that the biological activities of childbirth and breastfeeding, by challenging body boundaries, can be felt as intensely conflictual). Masculine gender identity, on the other hand, is positional: an achieved, learned status. To become a man a male child must repress femininity within himself and denigrate it outside: the achievement of masculinity is contingent on the subordination of women. One lesson that follows from this relates to the point I made earlier, that only a change in the definition of masculinity will affect the cultural devaluation of femininity. Men must accept "feminine" (domestic) interests as their own.

Thematically, the domestic-public duality is accompanied by an ideological opposition between nature and culture. This notion is very widespread in social representations of femininity. Sherry Ortner, in the chapter following Chodorow's ("Is Female to Male as Nature Is to Culture?") outlines one meaning of this: once again because of motherhood, women are easily linked with nonhuman nature. Women's biology encourages a definition of them as closer to nature than men. "Nature" becomes "women's nature." What is not feminine is seen (by men) as cultural, social order as opposed to natural disorder. The appropriation of social order by men as *the* social order, culture itself, further legitimates the "natural" inferiority of women.

Whichever way you look at it, it seems, having children is woman's essential handicap, her original sin. To claim that the universal subordination of women stems from childbearing is perhaps stretching a point, yet as a metaphor it usefully indicates the *difficulty* (but not impossibil-

ity) any culture has in overriding the tendency for the biological possession of a uterus to be culturally transformed into the possession of secondary social citizenship.

Moreover, it is clear from these volumes that the conventional sociological/anthropological focus on the institution of marriage per se has deflected interest from the even more powerful institution of motherhood. Christine Delphy, presenting a new view in her "Continuities and Discontinuities in Marriage and Divorce" (*Sexual Divisions and Society*, pp. 76–89), contends that it is because the family is the place where women rear children that marriage is "the institution by which gratuitous work is extracted from a particular category of the population, woman-wives" (p. 77). From children's dependence flows women's dependence and their "commitment" to marriage as the most valid social context in which dependence may be exercised. Deliberate efforts to promote the disintegration of conventional gender role behavior, as in some communes, stumble on the same cornerstone of motherhood, as Philip Abrams and Andrew McCulloch explain in a chapter entitled "Men, Women and Communes" (*Sexual Divisions and Society*, pp. 246–75): "What happens when the constraints of legally enforced monogamy and the wage-work–housework division of roles are removed, is not the creation of an alternative system of personal relationships, but rather the making plain of the deeper obstacles to equality through which men and women in this society struggle to relate" (p. 272).

If motherhood prevents equality, childbearing must be a burden. But—the usual question—whose voice is speaking here? Social and economic circumstances—isolation, poverty, lack of help—may make mothers feel worn down by their children, but the view of motherhood as uninteresting, marginal work is largely that of the male social scientist (a point made by Raphael on p. 66 of *Being Female* in a discussion of van Gennep's The *Rites of Passage*). To identify childbirth and childrearing as sources of inequality—like poverty or inadequate health care—does not mean that their eradication is a worthwhile or necessary goal. Many feminists (e.g., S. Firestone in *The Dialectic of Sex*) have collaborated with male ethnographers in equating motherhood with handicap and have outlined a vision of technological reproduction as the only true liberator of women (see H. Rose and J. Hanmer's "Women's Liberation, Reproduction and the Technological Fix," pp. 224–48 in *Sexual Divisions and Society*). And yet many mothers see their ability to give birth to, feed, and care for children as the greatest achievement of which they—or anyone else, female or male—are capable. (This insight can be extracted from many studies of marriage; see, for instance, L. Rainwater, L. P. Coleman, and G. Handel's *Workingman's Wife*, 1959; and H. Z. Lopata's *Occupation Housewife*, 1971. See also A. Oakley, *Becoming a Mother*, forthcoming.) To see childbearing as a resource rather than as biological and social weakness is a necessary correction of sexism both within and outside social science. The denial of all sex differences, part of the intellectual armory of femi-

nism, is a conceit of patriarchy, an overreaction of the underprivileged whose vision of themselves must necessarily undergo a long and painful, but ultimately fruitful, gestation.

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Book Reviews

L'Acteur et le système: Les contraintes de l'action collective. By Michel Crozier and Erhard Friedberg. Paris: Editions du Seuil, 1977. Pp. 445. Fr 65.

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The first task attempted by this book is one that was done for the American tradition of organizational analysis by Everett Hughes, Howard Becker, and Erving Goffman. It takes about 100 pages to say roughly that: (1) personal power arises in organizations only if both the person who exercises power and the person dominated might have chosen other lines of action, and consequently all theories that picture organizational action as determined underemphasize power phenomena; (2) there are nevertheless some very strong normative and realistic constraints on that freedom from which power arises, which tend to appear phenomenologically as "rules of the game," or "situational definitions," or the like; (3) at least on important matters, and to a surprising extent on trivial matters of face and small privilege, people play these games within the constraints fairly rationally, so that once one understands the rules of the game and the more or less standard motives of the people, one can explain most behavior by strategic considerations.

There is a terrible tendency in ethnomethodology and in *L'Acteur et le système* for this approach to lose its lightheartedness and irony and to become ponderously pedantic, with tedious definitions of intuitively obvious concepts. While it would perhaps be harder to do in French than in German or English, one is constantly tempted to tell Crozier that, after all, his maintenance men in the tobacco monopoly were just fooling around. This book refers to the previous work of both authors as what "we" think, have said, etc. A count of the number of times what "we" think comes from Crozier versus Friedberg would be as good an example as the overworked maintenance men, and more amusing.

What strikes me about it, then, is how something I entirely agree with can be made so irritating and pompous that I feel out of character when I believe it. I did not feel out of character when I believed Hughes or Becker or Goffman.

But even with Hughes and Becker and Goffman, there was always the question of whether there was not a bigger game—league standings, perhaps—in which these little games were embedded. The incentive pay game in factories is embedded in the big game of how hard workers will work for capitalist enterprises and how much of the product should go to profit,

how much to maintaining the bourgeois political system, and how much to what the workers themselves choose—see Michael Burawoy's forthcoming work on this general subject, *Making Out on the Shop Floor: Changes in the Labor Process under Monopoly Capitalism* (Chicago: University of Chicago Press, 1979). In Goffman, how much of mental hospitals is a production of total institutions, how much instead is what we would demand of whatever social arrangements we would tolerate to deal with lunatics? In Becker, how much of the authority of principals and teachers is due to the fact that kids really do not know a lot of things educated adults know, and how much is a situational device of the school that could well have been otherwise? Or, more generally, how far are the rules of the little games we play in our roles really explained by those terribly criticizable "functional requisites," which we always argue against except when we are making rules for the institutions where our own values are supposed to be institutionalized. We may be doubtful that societies have functional requisites, but we all make speeches every other week about functional requisites of sociology departments. Whether we speak truly is of no consequence in Becker and Goffman and Crozier and Friedberg. Perhaps our lack of interest in functional requisites is more convincing in this book, because we do not really care whether the monopoly makes enough cigarettes, so we can't get interested in the functional requisites of that task.

But the question we used to pose in our hearts to Goffman ("But what if the patients really are crazy?") has its milder form here ("But how many cigarettes do they really produce?"). The game metaphor starts with the presumption that there is nothing serious going on here, or with the irony that people take winning games more seriously than they do real life. I think we will never understand the other half of the irony of organizational life unless we admit that all those mediocre bureaucrats, on the other side from our guys, are really trying to educate kids and care for the crazy and make cigarettes.

The main point of the second major part of the book seems to be that organizational structures are set up by people and that they set them up the way they are on purpose. That means that any correlation between environment and organizational structure is a choice, a strategy, a human creation. That is, of course, what Philip Selznick and Alfred D. Chandler were trying to teach us, and it is true that the fashion in which Peter Blau, and to a lesser extent Derek Pugh, have tried to make us pretend otherwise in order to be "more sociological" is pernicious. But what this does is to make each of those opaque correlations into a problem: What are people thinking about when they regularly, for example, concentrate authority in the local executive if the local establishment is part of a larger firm, but decentralize authority locally if the local establishment is the whole enterprise? The reason the correlations in the back of Chandler's book are bigger than most of Blau's is that Chandler figured out what to correlate by studying what people thought, what strategies they were following. And I suppose Selznick could have produced correlations with the political

structure of regional authorities if he were not faced with some limitations on the sample size of TVAs. But Crozier and Friedberg do not offer a single explanation of any correlation from the structure-and-environment literature. Theoretically empty statements—for example, that maybe the reason the correlations hold up in several cultures is that they are "not important" or "purely formal" (p. 125)—do not get us anywhere. If people think alike in the same situation in different countries, as shown by these correlations, a book on "the actor and the system" ought to be able to explain it and ought not to wander off into sentimental cultural relativism at precisely the point where the scientific crunch comes.

If I had to guess where the faults of this book come from (the book is by and large right), I would say that they come from the authors' feeling that theoretical originality is a literary effect rather than an illumination of the world. Thus, if we strain long enough to put organizational processes into crisp language, it must be an advance. But without a sure touch, such a way of writing a book degenerates into a manner, and the book comes out too long.

Therefore I will not review the last two-thirds of the book, though I have sort of read it. It is more of the same—good, solid sense stretched out too long. One eventually learns a good deal about French public administration.

Rational Behavior and Bargaining Equilibrium in Games and Social Situations. By John C. Harsanyi. New York: Cambridge University Press, 1977. Pp. x+314. $29.95.

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A finite game is a model of a decision situation involving two or more actors (players), each of whom must choose one of a finite number of courses of action (strategies) available to him. The simultaneous choices of strategies by all players determine an outcome to which each player assigns a value (utility). A central problem in game theory is that of singling out choices of strategies that can be defended as "individually rational" in the sense of maximizing one's own utility (or expected utility) while taking into account the circumstance that all the other players are also rational, that is, are attempting to do the same. In this way game theory can be regarded as an extension of the normative theory of decision to situations involving conflicts of interest.

Games are classified according to the number of players, according to whether the players can coordinate their strategies to pursue common goals (form coalitions), and in many other ways. Each class of games presents a problem not only of finding optimal strategies (which is a computational problem), but also of defining them in a way to fit the structure of the

game (which is a conceptual problem). For example, in order to define an optimal strategy in a general two-person constant-sum game, the set of available strategies must be conceptually extended to include mixed (probabilistically chosen) strategies. A pair of optimal strategies is said to be in equilibrium: neither player can increase his expected payoff by choosing another while the other chooses his optimal strategy. Such an equilibrium constitutes a determinate solution of the two-person constant-sum game, in the sense of a unique apportionment of expected utility payoffs.

The task that John Harsanyi has set for himself is that of defining a determinate solution for every class of finite games. This task is beset with formidable difficulties, particularly in the context of two-person non-constant-sum games and in noncooperative games with more than two players. Although every such game has been shown to possess equilibria, the problem of singling out a determinate solution among these equilibria remains, as well as the problem of establishing the individual rationality of the choices that intersect in that equilibrium.

The solution of the problem necessitated a modification of the concept of a noncooperative game. The noncooperative game is usually defined as one in which players cannot communicate with each other and hence cannot coordinate their strategy choices to mutual advantage. Harsanyi regards a game as noncooperative if the players cannot make enforceable agreements on coordinated strategy choices. As for communication, it can take place even in noncooperative games in the form of "tacit bargaining," guided by the players' perception of each other as rational actors. This tacit bargaining, based on comparing the risks associated with the failure to agree, leads to a determinate solution. Since in the context of cooperative games determinate solutions reflecting collective rationality (appropriate in such games) have already been proposed (the Shapley Value being an example), Harsanyi's contribution rounds out the formal theory of rational decision in conflict situations.

Harsanyi emphasizes the importance of the rounded-out theory in applications to social science. He writes, "only a theory providing determinate solutions can suggest reasonably specific *empirically testable hypotheses* about social behavior and can attempt to *explain* and to *predict* . . . the outcome of social interaction in various real-life situations" (p. 4).

Here I must disagree. I do not believe that the results of game-theoretic analysis can be used either to explain or to predict behavior in real-life situations. I will go further and maintain that not even the normative aspects of game theory, that is, its prescriptions of how rational actors *should* behave in conflict situations, can be exploited in real-life conflicts. The gap between the game as a model of conflict and real-life conflicts is too enormous to be bridged. It is sufficient to assess what is involved in an empirical determination of cardinal (as opposed to ordinal) utilities of unquantified outcomes to realize that these procedures can be conceived as at best "thought experiments," impossible to carry out with real actors. Over and above these almost insurmountable obstacles is the circumstance that the tacit bargaining that is shown to lead to determinate solutions of

noncooperative games is beyond the capacity of any individual or organization.

The great value of the theory presented in *Rational Behavior and Bargaining Equilibrium in Games and Social Situations* is to be sought not in its descriptive, or its predictive, or its prescriptive aspect, but rather in its potential for enlightenment. Harsanyi builds the concept of rationality from the ground up with admirable clarity and rigor through a large variety of decision situations from those involving one actor to two-person and *n*-person games, cooperative and noncooperative. He treats exhaustively the logical structure of rational bargaining and devotes special attention to paradoxes that lurk everywhere on the path of strategic analysis. Particularly illuminating is his distinction between rational behavior and moral behavior.

In short, Harsanyi brings the full power of mathematical rigor to bear on the analysis of some fundamental concepts of philosophy, psychology, economics, and political science. The mathematical definition leaves no doubt about what one means by a term; the mathematical proof leaves no doubt about the validity of deductions. Thus, two fundamental requirements of a precisely scientific approach to a field of inquiry are fulfilled. The third requirement—empirical validation—is not relevant in the present context for reasons stated above.

A rather high degree of mathematical literacy (but not of technical knowledge) is required to read this book. There is no substitute for this faculty. The social scientist who wants to get some idea about the potential contributions of game theory and related fields to a theory of "rational conflict" will be well advised to learn to read the language, without which the contributions could not have been made, if only to protect himself against much obfuscation and nonsense that has been written and said on the subject.

Sociology and the Demystification of the Modern World. By John Rex. London: Routledge & Kegan Paul, 1974. Pp. xi+271. $14.00.

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John Rex thinks that the vocation of the sociologist is, as he insists throughout this book and reiterates in its penultimate sentence, "to demystify the ideology of revolution as well as the ideology of the *status quo*" (p. 253). His argument is generally persuasive, and he is altogether candid and forthright in rejecting alternative conceptions of the sociologist's role, including those of the detached scientist, the policy adviser to princes, the phenomenological anatomist of everyday life, and the *engagé* partisan of radical change. But the term "moral nihilism" that Rex chooses to describe his position is inappropriate. "Moral nihilism"—a pleonasm, surely—is too

strong with its echoes of Nietzsche and the passionate debates of 19th-century Russian intellectuals. Rex is not even recommending the professed value neutrality of scientific positivism, let alone a satanic or Dionysian antinomianism, nor is he bewailing the lost certainties of the past. He wants the sociologist to be ruthless in scrutinizing all moral presuppositions present in sociological work, not in order to extirpate them but because "by destroying, even for a moment, the sacredness of the world view of priests and lay ideologists it opens up the possibility of true moral judgment which is not itself a sociological matter" (p. 9). He does not conceal his own moral stance, which is primarily one of sympathy with the peoples of the underdeveloped continents subjected to economic and military exploitation by both the capitalist and the communist powers.

The "demystification" of the title is also, I think, an unfortunate choice. Weber adapted the term from Schiller to describe the erosion of traditional beliefs and values, especially those of a magical and supernatural character, resulting from the rationalization of modern life, but in the 1960s the word was widely appropriated by radicals and Marxists both inside and outside the academy to legitimate their assaults on the chief agencies of rationalization themselves, to wit, technology and bureaucracy. Indeed, some of them seemed eager to *re*mystify a world drained of magic, mystery, and moral absolutes by the "instrumental rationality" of capitalism and bureaucracy. They were, of course, parroting Lukács and the Frankfurt School, who notoriously cannibalized Weber. Rex sees sociology rather than any class or political movement as the appropriate agent of demystification, but his use of the term is scarcely free from ambiguity. He regards the modern world as the disastrous outcome of the hopes of the Enlightenment, the original source of the Weberian process of demystification. But he also wants to demystify the demystifiers who retain a commitment to revolutionary utopianism. The word cannot bear the weight of these conflicting resonances and serves poorly as a titular description of Rex's project.

The first two sections of the book review the epistemological foundations of sociological analysis and present a substantive theory of social relations, social structure, and conflict that owes much to a careful reading of Weber and elaborates on the themes of Rex's 1961 book, *Key Problems in Sociological Theory* (London: Routledge & Kegan Paul). The third section applies the general concepts Rex has developed to the first, second, and third worlds. The fourth section explores the moral and political implications of the present condition of these worlds and of the sociologist's vocation "in a collapsing civilization."

The two opening sections are a valuable statement of Rex's theoretical position and display a proper skepticism toward current fashions that is rooted in a firm insistence that sociology, if it is to amount to anything, must be substantive and have something to say that illuminates the messy problems of the real world. His argument is dense and terse, insisting, against phenomenologists, that sociologists are not bound to actors'—or "members' "—meanings as the finishing as well as the starting point of

their analysis and that they must develop a conception of social structure that is comparatively grounded. Rex locates the position of quantitative research in this perspective, criticizing the excessive claims made for it without descending to the philistinism of those who chatter about "number-crunching" to justify their own innumeracy. There is a valuable discussion of the variety and versatility of Weber's use of ideal-type concepts and a defense, against Parsonian and Marxist functionalism, of a Weberian view of social structure as a mosaic of "structural bits" that do not necessarily cohere to form a unitary system. All of these themes are discussed at a level that presupposes considerable familiarity with sociological theory, the result being that, like other such efforts, the book fails to achieve the author's expressed hope of making the insights of sociology "available to the great mass of the people" (p. ix).

There is a wide hiatus between the theoretical sections and the review of the contemporary world in the rest of the book. Since the editors of *AJS* asked me to write a review fully four years after the date of publication—presumably because an earlier assigned reviewer was delinquent—it must be said that the book does not wear very well as a "tract for the times." Watergate and Vietnam have passed into history and proved to be rather less typical occurrences than Rex assumed in the early 1970s. Wars and military skirmishes in the third world have broken out, even in a communized Southeast Asia, that are clearly not the results of great power machinations. Black unemployment in the American ghettos has increased, but black violence and crime have diminished, as has white backlash against them. Algeria resembles Frantz Fanon's hopes for it less than ever, and Fanon himself, to whom the book is partially dedicated, now seems to have been the figure of a transitory historical moment, his main heirs the German Baader-Meinhoff and Italian Red Brigade terrorists. The impact of Solzhenitsyn's three-volume study of the Soviet camps has even persuaded some French erstwhile *marxisant* intellectuals to abandon Marxism itself as a totalitarian creed. Several European Communist parties evidently disagree with Rex's evenhandedness in deciding that the differences between the first and the second worlds favor the former despite the "evils" of capitalism.

Yet Rex's pessimism about the future is not necessarily refuted by his overreaction to the events of nearly a decade ago; more recent events, especially in Africa, the continent with which he is most concerned, tend partly to bear it out. But his partisanship on behalf of the nonwhite masses of the southern hemisphere leads him to excessive simplification and selectivity. That the third world is an ideological fantasy rather than a social reality has long been evident. Nor can one push too far Rex's equation of the multinational corporation and the exportable model of the Leninist party as parallel bureaucratic monstrosities. As Rex recognizes on occasion, the first world contains more than capitalism even with respect to its economic institutions and practices. And to identify the second world with the Party as its characteristic institution is to take far too abstract a view, ignoring the actual societies that Communist parties in power have helped

to create, societies from which far more people wish to emigrate or escape to the West, decadent and materialistic though it be, than vice versa.

Sociology and the Demystification of the Modern World has, however, the power of a moving *cri de coeur* combined with the intellectual seriousness of one who does not take lightly the issues posed by his vocation as a sociologist. Possessing a real concern for the peoples of the underdeveloped regions and those of them who have emigrated to Britain, Rex communicates the anguish he feels at being caught between the relative indifference and rapacity of Western elites and the facile attitudinizing of radical sociologists whose commitment both to the cause of the racially oppressed and to a serious view of the vocation of the sociologist is open to question. The grounds for his anguish have hardly disappeared since the early 1970s even if a rather more complex and modulated view of the relations between the West, the Communist nations, and the heterogeneous peoples of Asia, Africa, and Latin America seems indicated.

Ideology and the Ideologists. By Lewis S. Feuer. New York: Harper & Row, 1975. Pp. ix+220. $14.00 (cloth); $3.95 (paper).

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Ideology and the Ideologists attests to the constancy of Lewis Feuer's mission of antimythology. Invoked during the End of Ideology scrap, his aphorism declared liberal civilization to start "when the age of ideology is over." He still warns of the dangers to democracy of the dreams of ideologues. A new coinage, "dialectical personality," son of authoritarian personality, places one context of the jeremiad against barbarism in the postwar surge of liberal anticommunism and the image of left and right demonologies fleeing from a vital center to embrace extremism. "For the trails that ideologists have blazed have led to the fires of the Nazi crematoria, and to the Arctic wastes of the Soviet labor camps" (p. 191).

In *The Conflict of Generations* (New York: Basic, 1969), Feuer portrayed his curative sociotherapy "bringing to consciousness the irrationalities" of ideology (p. viii). The title and language suggest the influence of Freud and Feuer's fascination with oedipal jealousies. Yet he has been no remote, inscrutable Freudian figure, more a feisty scold, chastising the Berkeley Free Speech Movement for its parricidal fantasies. He recently savaged hermeneutics as a way of "projecting your ideological myth into texts laden with vacuous words" and borrowed Arthur Koestler's words to attack neo-Marxist dialectics as a "method which enables an idiot to sound extremely clever" (*Contemporary Sociology* 3 [July 1974]: 318). To be sure, Feuer's bite is undialectically clever. Incongruously, *Ideology and the Ideologists* relies on a gauzy Freudian exegesis suspiciously resembling hermeneutics.

Feuer divulges the eternal ingredients in ideology, the emotional a priori. The quest for "invariant structures" owes less to the cognitive structuralism of Lévi-Strauss or to Lacan's Freudian heresy than to a simple notion of the unconscious. Beneath the claims of Marxism, nationalism, and fascism, Feuer detects infantile longing and mythic projection. A biblical allusion expresses the emanationism: "In the beginning was the emotion; then came the idea and last the deed" (p. 79). The sociology of knowledge dissolves in the psychopathology of politics in an erratic, reductive, and engaging analysis.

Ideology is a special vice of intellectuals, a charter for their rule. Weapon of struggle, a cry of pain, mainly a symptom of repressed desire, it is "always an unconscious defence of generational revolt" (p. 82). The wish for social transformation sublimates the rage of the sons for the fathers. Every ideology contains a Mosaic myth. Heroic redeemers—Lenin, Senghor, Mussolini—supplant the elders and establish mystical *communitas*. A father of conservatism, Edmund Burke sensed the theme of castration among the revolutionary "children of their country who are prompt rashly to hack that aged parent in pieces" (p. 91).

Feuer paints the ideologue as a narcissist driven to create the world in his own image. Like Vonnegut's antihero, the ideologue aims to "project a world," but the discipline of science thwarts "the free projection of wishes and emotions" and "evokes a sense of the intrusions of the elders on the 'omnipotence of thought' " (p. 131). The visionary is guilty of bad faith, draping his utopianism in metaphysics, but truth is only decoration. "Virtually every philosophical tenet has been used at some time or other by every ideology" (p. 19). Doctrine flows from a law of generational fashion; the sons of Parsonian progenitors sire conflict theories. The rootless intellectual achieves a conversionary élan by identifying with "chosen people," whether they are blacks, Albanians, or workers. Such passion is disastrous for liberalism. The conviction of election promotes a sadistic propensity to suspend human rights and to sacrifice generations in the name of an exalted humanity. Feuer presents the intriguing thesis that ideology invariably generates anti-Semitism. The purifying impulse compells the ideologue to seek tribal unity, but the Jews resist incorporation into the primal community. They are bearers of an autonomous tradition of individualism and rationality, a liminal race which blurs the boundaries of communal loyalty. They must be annihilated. Jewish survival is thus a metaphor for the promise of civility.

Oddly, Feuer exempts himself from the demands of the balance and evidence he expects from the ideologue. On the strength of a tautology which confines ideology to fanaticism, he removes from analysis the origins of ideology in the suppression of change, conceptions of justice, and social arrangements. Feuer thus repeats the errors of those dogmatic collective-action theories which focus solely on pathology, anomie, and strain. The stress on psyche impedes the comparative study of the role of a variety of factors in shaping ideology, such as party institutions, cultural tradition, and state formation. By the same token, phenomenology and conflict sociology

illuminate a diversity of motives behind generational rivalry which Feuer ignores. The effect is to demote interest and power to the derivative materials out of which myths are fabricated, revolutionary politics to the stage on which psychodramas are ritually enacted. Clifford Geertz summons us back to a pluralistic universe. "Whatever else ideologies may be—projections of unacknowledged fears, disguises for ulterior motives, phatic expressions of group solidarity—they are, most distinctively, maps of problematic social reality and matrices for the creation of collective conscience" ("Ideology as a Cultural System," in *The Interpretation of Cultures* [New York: Basic, 1973], p. 220).

As a counterpart to his conceptual fixity, Feuer violates the premise of empiricism. He resorts to rash generalization and speculative insight. Consider his version of the New Left, which contains a potent dose of truth. A segment of the New Left was afflicted with a nihilistic contempt for liberal civilization, as Feuer charges. The 1967 New Politics Convention endures as a testimony to anti-Semitism, thuggery, and masochism. However, another segment shared the egalitarian ideals of their parents, not oedipal *ressentiment*, and invigorated political conversation by advancing plausible criticism of interest pluralism, corporate power, and American globalism. This segment of the New Left conformed to a democratic heritage which Feuer rightly cherishes.

Feuer's antimythology verges on a mythology of American exceptionalism. He reveres the pragmatism of Jefferson and Franklin and praises America as "a country singularly lacking in ideology" (p. 164) or a myth of historical destiny. This is quite amazing. The "great nation of futurity," America has been influenced recurrently by a millennial idealism and ferocious social movements. The myth of Redeemer Nation has helped to catalyze and rationalize imperial crusades and nativist binges. Our civil religion rests on a Lockean ideology so ingrained that its values of self-regulation and privatism escape public scrutiny. These dilemmas do not detract from the considerable attainments of American democracy. Ultimately, to grant this is to call on Feuer to adhere to his own canons of critical skepticism, to demand a measure of rigor from his passionate humanism, and to affirm the integrity of liberalism.

Social Change in the Twentieth Century. By Daniel Chirot. New York: Harcourt Brace Jovanovich, Inc., 1977. Pp. x+273. $5.95 (paper).

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Modernization theory is dead, declared Immanuel Wallerstein at the 1975 Annual Meeting of the American Sociological Association; we should let it "rest in peace" without regret and get on with the business of studying capitalism as a world system. Daniel Chirot certainly agrees, but he does mourn

one thing about the demise of modernization theory: for all of their blatant theoretical inadequacies and ideological misuses, the works of such scholars as Talcott Parsons, Karl Deutsch, and Gabriel Almond did encourage U.S. college students in the 1950s and sixties to take an interest in foreign countries. Since modernization theories have been discredited, Chirot fears, young Americans have tended to turn excessively inward, focusing exclusively on U.S. problems or on issues of personal life-styles. It is past time, Chirot believes, for social scientists interested in comparative macrosociology "to popularize an acceptable and interesting new synthesis" (p. 5). This is needed both to reawaken interest in the study of foreign societies and to encourage educated Americans to realize that "changes in other parts of the world influence American society, just as changes in the United States influence the rest of the world" (p. 6). For these reasons, Daniel Chirot has written a new textbook for comparative macrosociology, one meant "to provide in elementary form a theory of social change that connects changes in the highly developed world with those in less developed societies" (p. 6).

Social Change in the Twentieth Century covers a lot of ground with considerable skill. Arguing that the key fault of modernization theories was to see development and underdevelopment as due to forces purely internal to each society, Chirot instead situates countries in an international hierarchy of core, semiperiphery, and periphery, according to how wealthy, economically diversified, and powerful various countries are in the system as a whole. World capitalism was at its height in about 1900, Chirot argues: capitalist owners (along with privileged middle classes) dominated politics in the core states; core countries had colonized much of the non-Western world; and core investors owned much of the modern, export-oriented economic sectors of peripheral economies. Then the consequences of capitalist expansion generated instability and threats to capitalist hegemony. Economic and military competition among core nations, and between core countries and rising semiperipheral powers like Japan and Russia, led to world wars, depression, and fascism. Meanwhile, core domination unleashed cultural and political forces that led to the revolt of the colonized and semicolonial periphery, generating nationalist revolutions of varying types, which have finally created a phalanx of new semiperipheral states, both communist and authoritarian-noncommunist, determined to assert more independent and active control over the future development of their national economies. For both the earlier and the later phases of the 20th century, Chirot surveys typical stratification profiles of major kinds of countries within the world as a whole. He carefully presents basic facts about patterns of world trade and foreign investments, comparative levels of national socioeconomic development, and statistics on military expenditures. Recurrently, Chirot discusses the prospects of political democracy, its changing meaning, and its suitability (or not) for countries in various international positions. Similarly, a refreshing amount is said about patterns of ideological and cultural stratification and how these have helped to promote changes within nations and on a global scale.

At first glance, *Social Change in the Twentieth Century* seems to offer a "Wallersteinian" analysis. But, in fact, Chirot differs from Wallerstein in crucial ways. Plainly, there are contrasts of political outlook. Whereas Wallerstein seems almost glibly optimistic that the crises of capitalism in the late 20th century presage the coming of a socialist world government, Chirot fears that core capitalist powers, desperate about their waning control over the old periphery and about the rising competition of the communist giants (USSR and China), could evolve toward new forms of fascism. And the only brighter alternative projected by Chirot is for "enlightened isolationism" on the part of a capitalist core newly willing to leave the less developed nations alone to pursue their own economic and political strategies. Associated with such political differences lie important differences in theoretical emphasis. Chirot's approach is more "world historical" than "world systemic"; for Wallerstein, core, semiperiphery, and periphery are enduring structural parts of an all-encompassing world capitalist economy, but Chirot treats these as categories that emerge and shrink over world time. By the mid-20th century, Chirot sees hardly any genuinely peripheral countries; instead, he prefers to speak about the capitalist core and the new semiperiphery, and about the coexistence of world capitalism with communist powers that have closed themselves off, relatively speaking, from capitalist influence (though they have not succeeded in creating an alternative international system). Much more than Wallerstein, Chirot portrays genuine interplay between intrasocietal and intersocietal changes. One gathers from Chirot that changes within countries have done more than allow particular societies to move from one place to another in a fixed international hierarchy; rather, they have "acted back" to change the very structures of international relations. Finally, and perhaps most decisive, Chirot places much more explanatory weight on patterns of political organization (i.e., of national states and revolutionary movements) and on international military competition than would any orthodox Wallersteinian.

Unfortunately, Chirot never raises for explicit discussion his theoretical disagreements with Wallerstein. Indeed, the chief weakness of *Social Change* is its theoretical elusiveness, an ironic fault for a work intended to popularize a new explanation of social change. Partly, the difficulty lies in Chirot's unwillingness to enter existing theoretical debates at a sophisticated level. Not only are differences with Wallerstein glossed over, but Chirot never confronts the arguments of "revisionist" modernization theorists such as Samuel Huntington or S. N. Eisenstadt. Nor does he exploit the theoretical contributions of non-world-systems critics of modernization, such as Alexander Gerschenkron, Reinhard Bendix, and Alejandro Portes. Chirot's own theoretical categories and generalizations cover the sociohistorical realities he narrates only very loosely at best. For example, the British settler colonies (Canada, Australia, New Zealand) are supposed to be "peripheral" (ca. 1900) but exhibit glaringly inappropriate socioeconomic profiles. In addition, much of *Social Change* revolves around accounts of wars, colonization and decolonization, and political revolutions, yet

Chirot never offers any rigorous hypotheses linking these to capitalist economic dynamics. He himself realizes the problem: Leninist hypotheses, derived from the theory of imperialism, are recurrently raised by Chirot as possible explanations for economic-political linkages. But, invariably, Chirot concludes that Leninist arguments do not work objectively (in terms of available data), even though nations "act as if" they believed Lenin's theory were actually true. With this cute paradox, Chirot leaves the theoretical issues unresolved. Knowledgeable readers, if not undergraduate students, will likely conclude that Chirot has dodged the toughest explanatory problems.

In a sense, this is a premature textbook, no matter how badly something of the sort is needed. Textbooks, almost by definition, have a smooth, authoritative air about them. Yet when it comes to rigorously explaining social change in—and of—capitalism on a world scale, all that can be validly done at this point is to raise issues and probe alternative hypotheses. Chirot could have offered significantly more along these lines; he could have produced a more open-ended, intellectually challenging introductory work. Nevertheless, *Social Change in the Twentieth Century* is a very helpful book; used in an explicitly critical and exploratory manner, it will allow teachers to raise the important questions about macrosocial change for their students to ponder with renewed interest.

Interethnic Relations: An Essay in Sociological Theory. By E. K. Francis. New York: Elsevier Scientific Publishing Co., 1976. Pp. xx+432. $22.50.

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Reasoning inductively from an explicitly limited range of case materials, E. K. Francis sets out to construct an architectonic theory consisting of an interlocked set of 103 propositions clarified by 59 definitions that would explain "the ethnic aspect in social behavior" (p. 6). This work, abuilding for several decades, is the culmination of the author's career and grows out of his earlier essays, *Ethnos und Demos* (Berlin: Duncker & Humblot, 1965). Francis's central theme "concerns the contradictions between the principles on which the modern nation-state is based and its inability to solve the ethnic problems that these inconsistencies have engendered" (p. xvi). He divides the body of his work into four parts (and 26 chapters): the ethnic factor in social interaction, "ethnie" and polity (wherein the neologism refers to a wider grouping than an ethnic society), interethnic relations in industrial society, and ethnic relations in colonial situations. In part 5 Francis sets forth his "outline of a theory." He aims no less than

"to arrive at a comprehensive set of interlocking empirical generalizations on the basis of evidence supplied by four social sciences [anthropology, history, political science, and sociology], and selected from three continents [central and western Europe, North America, and sub-Saharan Africa] and different historical periods" (p. xii). Francis should not be faulted in this ambitious task for failing to consider ethnic problems and circumstances that beset the other half of the world's populace, that is, experiences in eastern Europe (especially in the USSR and in other socialist countries), in Asia, in the Middle East and the Maghreb, in Australasia and the Pacific, and—save Mexico—in Latin America and the Caribbean. The author builds from a consideration of simpler settings in which the ethnic factor is at play (among the Nuer, the Tswana, the Swazi, the Zulu, and the Ganda), moves on to ethnic groups that cohere in modern (industrial) societies (e.g., the Jews in Minneapolis), goes on to take up the special situations of ethnic groups in nations newly formed out of decolonized cloth (Nigeria, Mexico), and concludes with a consideration of racial segregation and nationalization in the Republic of South Africa.

One of Francis's main distinctions is between primary and secondary ethnic groups. Primary groups focus upon descent and retain cohesion even when their members become subject peoples. His range of cases includes the Manitoba Mennonites, the Mormons, the New Mexico Hispanos, and the South Tyrolese Germans. Signally, the particular situation of the growing Chicano population in the United States finds no place in Francis's work —but then, many a reader will find his pet case absent. Secondary ethnic groups include, in the main, groupings of European migrants to the United States. He considers how well—or poorly—they have been able to maintain their ethnic identity and distinctiveness in the American caldron. European parallels are then drawn (with an emphasis on the Poles in Belgium, in the Ruhr Valley, and in Great Britain) to modify the theorems. Plainly, the author is attempting to specify the sweeping changes members of European ethnic groupings have undergone in the move from tradition-bound to modern, industrial societies. And once the migration has been accomplished, he focuses upon "the structural integration of new additions to a modern society" (p. 295). By integration he means the extent to which members of the migrant ethnic group "function properly within the social order of the host society" (ibid.). Even black Americans are fitted into the category of secondary ethnic subgroups (pp. 283–87). Francis ostensibly uses an equilibrium model in reviewing the fates both of ethnic and racial minorities in Western societies and of decolonized or still subjugated peoples in Africa. He criticizes the "pluralists" (like L. Kuper, M. G. Smith, P. van den Berghe, V. Rubin) who alternatively stress ethnic or racial conflicts that lead to shifts in power (pp. 368–76).

To Francis, the Jews, surely history's classic minority group, present but a "taxonomic problem" (p. 158). Sorting American Jewry by point of embarkation and by recency of arrival, he concludes that while the hordes sprung from the ghettos of Eastern Europe cohere ethnically, those of eman-

cipated, German origin are merely adherents of a religious confession. The point is arguable.

Francis's overall effort at constructing an ethnic taxonomy deserves careful study. Even so, his tightly knit scheme will require modification when conditions outside of his purview are taken into account.

The Ecological Transition: Cultural Anthropology and Human Adaptation. By John W. Bennett. New York: Pergamon Press, 1976. Pp. xi+378. $16.50 (cloth); $9.50 (paper).

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This is an ambitious work of wide scope. It incorporates anthropological materials on small-scale societies, historical studies, and materials about large-scale industrial societies. It draws ideas from biology, systems analysis, anthropology, and other fields, and much of the presentation is at the level of general theory. It is prescriptive as well as theoretical in that the author suggests a path for anthropology to become more useful in the solution of contemporary problems. It appears that the intended audience was professional anthropologists and advanced students, but the book is of sociological interest as well.

Although Bennett calls the work a series of essays, it is a unified volume with a central theme and without internal duplications. It includes chapters on human and cultural ecology, systems and ecosystems, the use of biological concepts, and adaptation as social process.

Bennett offers two principal definitions (p. 156) of cultural ecology, a central concept. The first definition says that cultural ecology is the study of the processes in one major feedback loop in society. The loop involves physical environment, technology, social organization, and "presses," which include values, needs, goals, etc. (fig. 1, p. 38). Bennett emphasizes that the special concerns in ecological analysis are environment, particularly physical environment, and energy-conversion processes. The loop represents "the way the energy-conversion process is related to social and psychological components of the total system" (p. 156).

The policy-oriented definition of cultural ecology states that it is "that field which examines the consequences of human actions toward the physical environment for the environment and for humans, with a view toward modifying or controlling these consequences" (p. 156).

The ecological transition mentioned in the title is "the progressive incorporation of Nature into human frames of purpose and action" (p. 3). It includes the trend toward large-scale societies with population growth and toward mechanization and exploitation of natural resources.

Unlike sociological ecologists, Bennett incorporates values into his ana-

lytical framework as an element of culture. He says that the concept of culture includes values and may include moral precepts "which function as constraints on free choice of models for action" (p. 273). This idea links with his emphasis on adaptation as the criterion for evaluating human ecological arrangements. He presents a master criterion of adaptivity, "sustained yield," which refers to the survival value of the ecological pattern for the collectivity. Beyond this, he offers more specific criteria, such as the biological and psychological well-being of the population as well as the continued good condition of the environment itself (pp. 298–99), recognizing the potential contradictions among the criteria. He encourages anthropologists to apply values and to study the alternative outcomes of ecologically relevant choices.

Bennett is well qualified to attempt to build a cultural ecology, based upon his previous research. He has studied agricultural practices and social change in diverse settings including North America and Japan. The book is grounded in the anthropology of food production and more generally in economic anthropology. The author does not overemphasize his own published work, though, and the volume includes the work of numerous colleagues.

The summaries and critiques of many anthropological works will prove useful to sociologist readers. These summaries deal with two ongoing debates, one on the treatment of environment in anthropological analysis and the other on materialist approaches to social phenomena. Problems with environment as a variable date back to deterministic theories, and Bennett traces the subject through possibilism before reaching his own proposal. The summaries and critiques treat the works of F. Boas, A. L. Kroeber, J. Steward, F. Barth, A. Vayda, M. Harris, and many others.

Bennett's ecology is self-consciously connected with biology. The science of ecology generally emerges from evolutionary theory, which emphasizes adaptation and survival. Even though these concepts should be central to human ecology, they have not been explicitly considered in recent work. Bennett grapples with adaptation and his discussion merits consideration. He also considers the utility of biological concepts in social analysis. He recognizes that bio-ecological ideas have been taken haphazardly from their origins and applied by analogy only. Studies of energy transformation in small-scale societies without high technology represent social scientific analyses which are more consistent with basic ecological ideas.

The Ecological Transition is not flawless, of course. Sociological human ecology gets rather short shrift as Bennett summarizes related disciplines. His conception of sociological ecology is the study of the "geographical distribution of social components in space" (p. 71). This neglects the more general concern with community in sociological ecology. His discussion ignores the line of studies on sustenance organization and division of labor. It also neglects other recent materialist work in sociology.

Bennett omits the built environment as an object of investigation. His scheme shows the physical environment—physiography, climate—as a setting for social life. He neglects the study of vernacular architecture which

shows how people build their own environments, which in turn affect the conduct of social life.

The bibliography is extensive and interesting, containing more than 700 items. There are some errors in it, though: Fred Cottrell appears as Leonard Cottrell and Murray Edelman becomes Maurice. The footnotes are numerous and lengthy but citations in the notes are not cross-referenced in the index.

These are minor complaints about a first-rate book. Human ecologists will find it useful in helping to integrate culture and values into ecological analysis. It also provides an excellent overview of ecological, environment-based, materialist concern in anthropology.

Bureaucracy. By Guy Benveniste. San Francisco: Boyd & Fraser, 1977. Pp. xx+247. $9.95 (cloth); $5.95 (paper).

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Guy Benveniste argues that (1) bureaucracies do not work well because (2) they are attempting to relate to increasingly uncertain environments which (3) create excessive fears in their participants causing them to (4) react in a number of defensive and unproductive ways. The first third of the book is devoted to the amplification of these arguments with illustrations drawn from varied types of organizations. A number of games (strategies) played by individual participants and by organizations to insure their own safety and survival are described, as are other games played by client groups in reaction.

The second section is devoted to a description of several current remedies for organizational malfunctioning. The remedies discussed are organizational development (OD), decentralization and increased participation of lower participants in decision making, and the creation of special roles such as ombudsman. Each remedy is judged inadequate to deal with the real sources of the problem. Organizational development approaches deal with individual organizations, not with the environment of organizations. Increased participation is meaningless unless accompanied by genuine decentralization, and this is unlikely to occur to the extent that "centralization emerged because of a need for coordination and integration to reduce environmental uncertainty" (p. 166). And the ombudsman concept can deal with only a limited range of types of organizational errors and, in addition, provides no protection for the organization and its participants.

A final section is frankly speculative and attempts to peer into the future of bureaucracies. Two proposed general ideologies for dealing with organizational issues are reviewed: Large-scale societal planning is contrasted to contemporary self-conscious attempts to reduce the scale of organizations and to simplify social forms. Benveniste notes that planners reduce uncer-

tainty only when they plan correctly and that the movement toward self-sufficiency must either await the development of new technologies or accept a vastly reduced life-style. He sketches out a third schema for reform which involves the creation of a rather complex set of new institutions for oversight and regulation of organizations. Benveniste's admittedly utopian "New System" is composed of boards of professional governors who will protect and evaluate individual careers, councils to protect individual organizations from uncertain environments, professional courts to protect clients and the general public, and professional boards to raise the funds necessary to operate the system and to protect all of us from the new layer of bureaucracies created.

While there are some interesting sections—as, for example, a discussion in chapter eight of the uses and abuses of evaluation research—the argument as a whole is not convincingly made. The key concepts are not clearly defined or consistently used. No evidence is presented documenting the failure of organizations or the extensiveness of fear among organizational participants, or the prevalence of organizational client games. Assertions are made and, often, colorfully illustrated. They are not developed or conditionalized. We are not asked to consider what types of participants or which kinds of organizations employ what types of strategies to survive. In this sense, the great majority of assertions and conclusions proposed in *Bureaucracy* are so vague and general as to be virtually impossible to prove or disprove.

On the positive side, while I found the specific reforms proposed by Benveniste likely to generate more problems than they would solve, the generation of specific alternative futures is an art too little practiced by sociologists. I found the final chapters a refreshing alternative to the conventional sociological denouement: the call for additional research.

The Inheritance of Economic Status. By John A. Brittain. Washington, D.C.: Brookings Institution, 1977. Pp. xiii+185. $9.95 (cloth); $3.95 (paper).

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Let me begin by giving away the ending: John Brittain makes a strong argument that the major determinant of economic inequality is not luck but socioeconomic background (SEB). He also shows that years of education play a strong role, both independently and as an intermediary between SEB and economic status.

Just as he is not a supporter of the "luck" school of inequality, neither is he a meritocrat: That is, he is disturbed by his findings, even if income differences are due to real differences in productivity among people. Nor

is he swayed by arguments that our economy offers individuals equal opportunity to exploit their various productivities.

As long as I am praising negatives, let me add that this book does not address the question of genetic versus parental environmental components of income determination, but not because Brittain shares Bowles and Gintis's view that intellectual skills are not the prime determinant of income. Brittain is not much concerned with the process of education, what is learned in schools, or with socialization processes in school.

This is because Brittain seems to share, in part, the neoclassical economic view of income determination as the result of an endowment of skills and property, although the extent to which incomes are similar to parents' income is due to "the degree of privilege conferred by one's socioeconomic background and other parental influences" (p. 1). Also, his data set does not provide access to answers to questions about nature versus nurture any more than it tells us whether education is "a screening or signaling device to employers" (p. 2) or something that actually changes the students.

This last criticism is only meant to indicate the scope of *The Inheritance of Economic Status:* it is a valuable examination and measurement of the effects of socioeconomic background and parental influence on economic position, not an analysis of the actual processes of upbringing through personnel office procedures years later.

The study is limited in one other way: the sample used. The sample is 659 parents who died in the Cleveland area (Cuyahoga County) and their children, but I would not suggest retitling the book *659 Dead People from Cleveland.* Use of probate records provides hard to come by data while limiting the scope. I have extracted a national sample of parents and children from the University of Michigan Income Dynamics Panel, but it suffers from the young age of the second generation. Brittain argues that his sample is typical, except for religion (majority Catholic), the large size of families, and the origins of the parents (majority immigrant).

On the other hand, the data refer to actual sets of brothers. Brittain makes much of this superiority to Jencks's data, whose central result was a low association between parents' and children's incomes. Jencks did not have actual data on brothers and was forced to infer the correlation between brothers' incomes from equations regressing income on a limited list of background variables. This, of course, biases the Jencks results downward. To the extent that components of background are left out, the effect of background is underestimated.

Brittain is able to compare the variance in incomes of brothers to the variance in the entire sample, partitioning inequality into the part caused by differences in background and the part caused by other effects or luck. Brittain's finding is that family effects account for 34%–61% of the variance in economic status; 34% for a crude measure of occupational status, more for income, and 61% when a composite measure of economic position is used.

Brittain presents essentially the same results in a number of ways. One striking result was that all the sons from the top tenth of the index of

background achieved greater economic status than all the men from the bottom tenth. So much for mobility.

The above results rely on the comparison of brothers to the entire sample and assume that brothers have in common only their background. To the extent that they imitate each other, form partnerships, and so on, the effects are not due strictly to their parents. But then, all these effects are due to the fact that the men are brothers.

Examining particular background variables allows the assessment of the importance of each measurable component of SEB and the expansion of the sample to single-child families. Brittain finds the effects of parental education (and age) to be strongest, although the strength depends on the measure of son's economic position. The effect of parental education is so strong that Brittain suggests it may be a proxy for genetic influences, as well, or for parents' income. My results using explicit measures of parents' incomes and educations together indicate the latter to be true. Brittain also examines parental occupation, family size, religion, and race.

I will deal with the section on the role of education more briefly. By the usual technique of regressing education on background and economic status on education and background, Brittain finds that one-fourth to 40% of the effect of background on economic position is transmitted by means of education (as well as by means of unmeasured variables to the extent that they are correlated with education). Also, he finds a strong independent effect of education.

But is not the effect of education exaggerated because of a more complete specification of education than of background, with which it is correlated? This problem is reduced by the use of the brothers sample, representing background with a dummy for each family. While these dummies do reduce the effect of education (and correlated variables) on income or on a composite index of status, education remains significant. The marginal effect of a college diploma over a high school education was on average a 24% increase in income, holding background constant. I would add that the importance of background to education would make wide differences in education unlikely for people from the same background.

I cannot let pass without comment the interesting half chapter on marital selection, the only section where women are considered. Brittain's finding is that the relationship between a son's economic position and his parents is almost duplicated by the association between his position and his parents-in-law, strong evidence for associative mating. Unfortunately, this section says little about the actual economic situation of women outside the home in the workplace, because the economic status of women is taken to be that of their husbands.

My final reservation concerns Brittain's statements on policy implications. He suggests various tax schemes to reduce the effects of inheritance of both wealth and productivity. I can only say that he seriously underestimates the importance of the reproduction of the distribution of income to maintenance of our economic system.

In summary, I would strongly recommend this book for its empirical

examination of the inheritance of the economic status of men. My disagreements are political; I would go elsewhere for an analysis of the actual processes of reproduction. The most important contribution of the work is to show that inequality is, in fact, inherited.

Industry and Inequality: The Achievement Principle in Work and Social Status. By Claus Offe. Translated by James Wickham. New York: St. Martin's Press, 1976. Pp. 158. $16.95.

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Written originally in German as a Ph.D. dissertation in 1967 and translated for English readers a decade later, *Industry and Inequality* presents an interesting and critical analysis of the validity of the achievement principle as a basis of stratification in modern capitalistic industrial societies. Offe argues that organizational conditions of work have changed in such a way that it is no longer possible to distribute rewards for work on the basis of individual performance. And although much institutional support is given to the achievement principle, it is no longer the basis by which rewards are actually distributed. The concept of achievement has become an ideology for justifying social differences rather than a mechanism for achieving social equality.

Briefly, Offe hypothesizes that modern industrial societies can be characterized by increased numbers of large, specialized, and highly technological organizations and that these characteristics have changed the basis of organizational structures from task-continuous qualifications to task-discontinuous qualifications. By these terms he means that those promoted to the next higher position in the hierarchy require different skills from their subordinates and are often recruited from different sources. This change in organization structure makes subordinates' actions invisible to superiors who are in turn incapable of evaluating performance variations.

These changes in organizational structure modify the workers' role in two dimensions. First, the impossibility of exercising formal interpersonal authority in task-discontinuous organizations requires that organizational control be exercised through internalized norms. Thus the proportion of normative regulative rules increases in proportion to the number of technical rules in workers' roles. Second, specialization and technology have increased the proportion of preventive actions and decreased the proportion of initiatory actions in workers' roles. These changes make performance evaluations impossible as content of the work role has become largely normative. According to Offe, norms are dichotomous. One can only comply or not comply. Moreover they are socially learned and cannot be considered an individual attribute. The individual content of workers' roles has disappeared.

The author validates his argument with detailed analyses of bases of variations in intraorganizational social status: promotion, recruitment, and labor income. In all three cases he finds that peripheral as opposed to central work-role attributes play the predominant role in the assignment of social status. This is especially evident in the case of allocation of labor income, where government intervention in the marketplace through wage actions and secondary programs for income equalization reflects society's shared cultural values more than the actions of individual recipients. Offe feels that relinquishing achievement as a basis of stratification will permit the development of more egalitarian social structures based on status rather than contract—all members of society determining the basis of allocating resources through shared beliefs and values.

Offe supports his arguments with data from both German and American empirical studies. His analysis is wide ranging and detailed and can only be partially presented here. But the arguments are never vague, and his analysis is developed with precision. There is much opportunity for debate on many levels. As he has chosen a central institution, and one seldom studied, as his subject matter, the study is both important and controversial. Scholars working in complex organizations, occupations, professions, and political sociology will all find new insights for their own work as well as new directions for research. The translator deserves special recognition for bringing this monograph to English-speaking audiences.

Comparative Union Democracy: Organisation and Opposition in British and American Unions. By J. David Edelstein and Malcolm Warner. New York: Halsted Press, 1976. Pp. viii+378. $19.95.

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Interest in problems of union democracy in this country reached its peak in the early 1960s following the passage of the Landrum-Griffin Act (1959). Edelstein, the senior author of this study and the American member of the team, did his major research at about that time. *Comparative Union Democracy* includes some later data, but fundamentally it rests on material now about 15–20 years old.

Edelstein and Warner introduce their book with a discussion of the nature of oligarchy in trade unions. Their catalog and annotations of American and British writers on this subject are quite complete. Their theory of union democracy follows. Although the subject is necessarily a complex of many variables—they emphasize the hierarchy of officials, regional substructures of the unions, voting systems, and electoral models—they choose to test the existence and degree of democracy in unions by tabulating the number of contested elections for top office and the number of resulting defeats of incumbents. They deal with these data

in two ways: statistically, and descriptively in case studies. The first approach results in a data study of 31 British and 51 American unions of given sizes as of 1952 and 1954, respectively. (They offer no explanation of why they chose a statistical base more than 20 years old, even though their case material covers the sixties and makes passing reference to even more recent events. A possible explanation is that they wanted to use—or had access to—the same data which the British scholar, V. L. Allen, had analyzed in 1954, but the study is not designed to replicate Allen or to compare the respective conclusions.)

Before undertaking the statistical analysis, Edelstein and Warner somewhat laboriously develop a set of hypotheses. Their tests of these produce no startling results. In Britain there are more candidates for top office than in America, more decentralization in union structure and power, less overt organization of factions, more use of proportional representation in voting, and more runoff elections. But are the British unions therefore more democratic? The authors find that "in view of the overwhelming weight of factors favourable to electoral opposition in British unions, it is puzzling that the British periodic elections are not much closer than the American, and that top officers are more rarely defeated in Britain than in the United States" (p. 133).

The case studies that make up the second part of the book are its better half. The two British chapters are classically good. They do not limit themselves to elections but reveal many elements of union life not anticipated in the hypotheses. A distinctive and recurring factor which seems decisive in Britain is the influence of outside political groups—Communist, Maoist, Labor, left-socialist—with which contestants are identified and from which they draw unmeasured amounts of aid and support. The unanswered questions are how much do these political groups account for a multiplicity of candidates and to what degree do they contribute to union democracy?

The case study of the British engineering union's elections shows that, while they have been actively contested and have often resulted in runoffs, they have engaged the participation of a very low percentage of the members—7.9% in two of the elections and not above 15% in 30 years. Participation may not determine the outcome of elections, but surely it is at least as good a measure of democracy as is the fact that elections are contested. The significance of participation is not hypothesized. One learns, however, that Edelstein and Warner prefer the referendum to balloting in convention and the postal ballot to casting votes at the union hall or workplace. To this extent participation has its place.

A factor favoring high rates of contest in Britain is determined to be the existence of fairly autonomous subdistricts within the union, where regional leaders develop constituencies and gain political experience. Unfortunately, the essay on American cases does not examine this factor, and we are thus deprived of an opportunity to understand the significance of this structural element in a comparison of the two systems.

The book as a while is diffuse. Its two parts are insufficiently connected and interwoven; and its concluding chapter, "The Future of Union De-

mocracy," moves far afield from the thesis in order to deal with such matters as humanization of work, worker-management, rationalization, and even to speculate on the role of unions under a possible democratic socialism. More serious is the questionable value of its basic thesis as a decisive indicator of democracy. On the positive side are the two British union case studies, the summary of writings on union democracy and oligarchy, an excellent bibliography, and a discussion of the differences in union structure and function between the United States and Great Britain.

Modeling Social Processes. By Patrick Doreian and Norman P. Hummon. New York: Elsevier Scientific Publishing Co., 1976. Pp. xvi+172. $10.50.

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In *Modeling Social Processes.* Patrick Doreian and Norman Hummon argue for the use of a systems-theoretic approach for the study of social processes which evolve over time or space. They distinguish among three components which together define a system. The units (or variables) are the individual pieces of which the system is composed. The structure refers to the relationships among the units of the system (including the distribution of the individual units), while the process is "the means whereby structures are changed or maintained" (p. 6). In this perspective special attention is paid to change and to the mechanisms which control change as the focal points for theory and research.

Using this systems approach, Doreian and Hummon provide a coherent rationale for the use of systems of differential equations to model the through-time (or space) trajectories of the units of the system. Systems of differential equations are ideal for many applications since: (1) They distinguish between units, structure, and process; (2) it is possible to incorporate information on the environment of the social system into the model; (3) once the parameters of the model are known, the dynamic properties of the system (projection over time, equilibrium/disequilibrium) can be studied; (4) the effects of a change in any variable in the system or in the environment can be predicted for the total system.

Doreian and Hummon present four examples of differential equation models. For each example they outline the theory underlying the particular model in conjunction with the mathematical specification of the differential equations. They then develop estimation procedures and discuss the properties of the model as given by the parameter estimates. Three of the models (labor turnover, organizational change, and government change) explicitly allow change only over time, while their model of political insurgency incorporates change over both time and physical space. However, all of the models are based on the authors' notion of "structural control." The basic idea behind this concept is that for each variable in

the system there is an ideal value at any point in time which is determined by the observed values of the other variables in the system at that time and by the values of the exogenous (environmental) variables. Change in the system, then, is generated by the discrepancies between the ideal and the observed values of each variable endogenous to the system.

There are, however, three important issues which the authors do not address; two concern their use of differential equation models and one concerns their estimation procedures. First, there is the issue of the appropriateness of continuous-time as opposed to discrete-time models, that is, of differential equation models versus difference equation models. For their example of labor turnover in particular, it is difficult to conceive of their endogenous variables (labor turnover and overtime) as changing instantaneously. Yet using differential equations to model changes in these variables explicitly assumes instantaneous change. Instead, it seems more appropriate to conceive of these variables as changing periodically (e.g., day to day) as step functions over time and to model the system using difference equations. System of difference equations have the same desirable properties as mentioned above for differential equations and are clearly appropriate where change can occur only periodically and not continuously (see also W. J. Baumol, *Economic Dynamics* [New York: Macmillan, 1970]; S. Goldberg, *Introduction to Difference Equations* [New York: Wiley, 1958]).

Second, the models which Doreian and Hummon develop based on their concept of "structural control" are mathematically indistinguishable from an alternate model. Suppose that a model is developed, on theoretical grounds, that states that the change over time in an endogenous variable, say, the first $[dY_1(t)/dt]$, is a function of the observed value at time t of that endogenous variable $[Y_1(t)]$ as well as of the observed values of the other $k-1$ endogenous variables $[Y_2(t), \ldots, Y_k(t)]$ and the values of the exogenous variables $(X_1, \ldots, X_n)$. That is,

$$dY_1(t)/dt = a_1Y_1(t) + a_2Y_2(t) + \ldots + a_kY_k(t) + b_1X_1 + \ldots + b_nX_n \,. \quad (1)$$

Using equations analogous to (1) for the other $k-1$ endogenous variables, a system of differential equations can be specified which is mathematically identical to the kind of models the authors develop. But the interpretation of the two models is quite different. For example, under the "structural control" model the parameter a_1 indicates the sensitivity for change in $Y_1(t)$ to discrepancies between the observed and ideal values of $Y_1(t)$, while under the alternate model, it describes a feedback mechanism for changes in Y_1 being generated by the observed value of Y_1 at time t. Thus, their "structural control" models cannot be tested against an alternate hypothesis of feedback as described above.

Third, there is an issue concerning their estimation procedures which poses a severe problem for their parameter estimates. While they do consider and resolve the problems which can occur if there is autocorrelation among the errors for a single endogenous variable, they do not consider the

problem of between-equation error covariance. Among other reasons, it is eminently likely that there will be between-equation error covariance, as this would capture effects on the endogenous variables which are unique for each observational unit. Then, given that each equation contains lagged endogenous variables which will be correlated with the error term and given the between-equation error covariance, an OLS or even a GLS estimation procedure for any single equation (which the authors employ for all their examples) will produce parameter estimates which are neither unbiased nor even consistent (see also H. Theil, *Principles of Econometrics* [New York: Wiley, 1971], chaps. 8–10). Thus, their estimation procedures produce parameter estimates which must be viewed with skepticism as to their accuracy.

In sum, Doreian and Hummon provide a good introduction to the use of differential equation models for social systems, but their work suffers from the neglect of several important issues.

Women in the Twentieth Century World. By Elise Boulding. New York: Halsted Press, 1977. Pp. 264. $15.95.

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"Running out of women" would be more serious than running out of oil, according to Elise Boulding's assessment of women's contributions to food systems. Women form a vast army of growers, transporters, sellers, processors, and servers of food. In Africa, up to 70% of agricultural production (p. 114) is done by women; elsewhere women's labor is the major transport system for water, the primary component of any food system. Both equity and progress demand that these socially invisible roles of women be recognized. Boulding's image of a nursing mother serving different meals to infant, small child, husband, and often festival guests while denying herself adequate nutrition is a powerful and poignant one. To those who might argue that progress demands such sacrifice or, apparently more benignly, that development cannot proceed on all fronts at once, Boulding makes a more difficult and significant argument. Because of their culturally different experience, women have special knowledge of local social terrains and the time and energy demands of innovation that planners cannot afford to ignore. Women also know, unlike socialist planners, that "the private shapes of individual lives cannot be fully mass-serviced" (p. 71). Women's resourcefulness and ability to meet individual needs from local materials are needed not only by developing countries. The world system as a whole needs to learn how to replace poorly functioning centralized systems with self-help techniques practiced by women householders.

Boulding's argument is presented in a series of 10 essays, six of which were issued previously and rewritten for *Women in the Twentieth Century*

World. The essays are part of a massive many-faceted project which has recently resulted in both the *Handbook of International Data on Women* (New York: Wiley, 1977), coauthored with Shirley Nuss, Dorothy Carson, and Michael Greenstein, and her own macrohistory of women, *The Underside of History* (Boulder, Colo.: Westview, 1977). Among the essays are Boulding's widely quoted "Women, Bread and Babies," as well as analyses of economic dualism, food systems, nomadism and urbanization, and familial constraints on women's work roles. The final section of the book changes from these socioeconomic topics to a consideration of women's international voluntary organizations (NGOs) and their capacity for acting as world peacemakers.

The reach of these essays is truly macrohistorical and global. Boulding ranges from prehistoric agro-villages and present-day nomadism through the impact of urban civilization to futurist planning in a world society. In the course of these discussions, she provides many arresting concrete insights which locate her analysis in the particularities of daily life, a style which well matches her argument. She alerts us, for instance, to the nurturant roles played by men in Laslett's 16th-century England, where families raised each other's children as craft apprentices, and to the communications function of hairdressers working among women. She further helps us to recognize Dickens's ragged children as the children of working mothers in a society which made no accommodation for women bearing family responsibilities alone.

The methodological orientation of the book is toward analysis of aggregate data, drawn largely from the *Handbook* of UN census data by Boulding et al. Boulding's two major interests are measuring the work done by women, both in the paid labor force and in the subsistence sector, and assessing women's contribution to the household. The difficulties of doing so are as prodigious as the urgency, both of which Boulding amply demonstrates. I find the results of her efforts, however, disappointing. She often presents too much information, overwhelming the data needed to support her analysis with statistics which prove to have no correlation. There are other occasional problems of presentation, including noncomparable tables (pp. 122, 126), misquoting data from tables (p. 123), and a confusing method of drawing data from more than one place to make a single point (pp. 122–24). The book would be easier to read if the tables had been redrawn for the purposes of this presentation and the supplementary data placed in appendices.

A more important concern, however, is the reliance on aggregate data. Boulding's analysis shows the insufficiency of aggregate data analysis, for her statistics often fail to illuminate her questions. This is most apparent in her discussion of the impact of economic dualism on women, in which she relies on the controversial indices of dualism developed by Adelman and Morris. After an extended discussion, Boulding admits that she finds no real relation in the data between dualism and women's labor force participation, because women show both high and low participation at every level of dualism. One senses with Boulding that there probably is a very

significant relationship somewhere. To find it, however, we must turn to detailed studies of relationships between classes and occupational groups within countries rather than summary indicators of whole societies.

Boulding's essays are significant because they help us to see things differently. She provides many arresting metaphors—for example, that working women are moonlighting when they return home to process food—which give concreteness to her very important analysis of women's contribution to food systems. The next step is an examination of the concrete processes that make up these systems in particular contexts in order to interpret the meaning of the statistics she reports.

Power and Pawn: The Female in Iberian Families, Societies, and Cultures. By Ann M. Pescatello. Westport, Conn.: Greenwood Press, 1976. Pp. xix+281. $15.95.

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Ann Pescatello has, by her own admission, chosen a task ". . . presumptuous in its temporal or its spatial dimensions" (p. xv): a survey of female roles in Iberian societies, past and present. In spite of its seeming unmanageability, she persisted, spending over a decade on the data collection and final report. Sadly, Pescatello's efforts offer limited new information and less new insight. Her study is disappointingly superficial. It is atheoretical, methodologically unsophisticated, and marred by careless editing and proofreading.

A review of the chapter headings immediately impresses the reader with the monumental scale of Pescatello's endeavor. In the first chapters, she deals with women's roles in Spain and Portugal before the conquest of the New World. Next she describes the social, economic, political, and religious contexts of the areas which underwent Iberian exploration—Asia, Africa, and Amerindia. The confrontation of native and European heritages during the conquest is briefly discussed; continued developments through the 19th and 20th centuries in all societies influenced by Iberian settlers are then summarized. Finally, one chapter is devoted to an examination of women's roles in 20th-century Brazil, before the book closes with an epilogue "in the manner" of a summary and conclusion.

Pescatello's sources appear in a massive bibliography, which includes works in at least half a dozen languages. Many of these are primary sources, predominantly reports of early travelers and colonizers. Others are secondary historical analyses. While some data are taken directly from censuses and government surveys, more often statistics are derived from official or scholarly summaries. Conspicuously lacking in the bibliography are recent books and articles (in any language) by other social scientists. The bulk of recent references consists of articles published in an earlier collection Pescatello edited. Although years of field experience are cited

as another source, the text contains meager evidence of her personal observations.

As a whole, the book is overburdened with broad generalizations and minute statistics which are never integrated into a meaningful argument. Pescatello informs us that prior to conquest "Africans had developed sophisticated social, political, economic and belief systems" (p. 87). We are told that "Uruguayan boys and girls apparently are taught sex roles at an early age . . . " (p. 203). In the way of unnecessary detail, we find a full listing of 1914 female employment figures for Argentinean cities, with no obvious reason for considering this dated regional breakdown.

Pescatello could have given us well-prepared, useful, summary tables of the pertinent information which is already present, but not easily accessible, in her text. For example, I would have appreciated tables showing national statistics on female employment, dates for the passage of important national legislation affecting women (e.g., the franchise, protective labor laws), and charts of the relative rates of progress through educational institutions for women and men in the countries studied. These data are now scattered through the text like pre-Columbian artifacts on an Andean mountainside; the reader should have been spared an archaeological trek in favor of a comfortable museum visit.

Interesting anecdotes and historical commentary dot the first few chapters. The traditional seclusion of upper-class Portuguese women is charmingly revealed in an old proverb: " . . . a virtuous woman left her home only three times during her lifetime: for her Christening, her marriage, and her funeral" (p. 157). Likewise the odd perception of Iberian colonizers is highlighted by the words of a Spanish bishop who, in 1566, marveled at the large breasts of Mayan women and attributed their special endowment to " . . . the continued grinding of tortillas [which] agitated their breasts . . . " (p. 124). The intriguing subjects of female slavery, female prostitution, and the growth of Catholic convents are treated in the first chapters. Pescatello portrays slavery and prostitution in both the indigenous and the Iberian cultures and then follows their natural emergence in the hybrid colonial setting. She explains how the confinement of nuns was often used as a form of population balance in societies with an excess of females. In other societies, she describes the competition between the Church and eligible bachelors over the few available European women with substantial dowries. These observations occasionally refresh the weary reader.

The most disappointing chapters of the book are the ones dealing with the contemporary period. It is disheartening to see Pescatello rely so heavily on area handbooks for information. The statistics from these and other secondary sources are inexcusably outdated. Here the need for current references from economics, sociology, anthropology, and political science is most obvious. Two works, among many others, whose absence is startling are E. Boserup's *Women's Role in Economic Development* (New York: St. Martin's, 1974) and N. H. Youssef's *Women and Work in Developing Societies* (Westport, Conn.: Greenwood, 1976).

In the contemporary chapters, Pescatello also fails to mention a few noteworthy issues. She hints that in Argentina and Chile there is some divergence from the strictly Iberian pattern of sex roles, but she overlooks the contribution of recent European immigrants to these national cultures. Even more glaring is her neglect of the revolution in fertility and family planning which has been fully documented for many countries of Central and South America.

Though Pescatello claims that " . . . in the figure of the mother and thus as 'head' of the household [the Iberian female] has been the central figure in the family and thus in society" (p. 231), there is scant information presented to support her contention that women are power brokers. Precisely to the contrary, she repeatedly offers evidence of the sexual and emotional imprisonment of women, of the exploitation of women as low-level workers, and of the legal limitations on the civil rights of women. She shows how remarkably persistent the legal definition of women's inferiority has been in Iberian societies. From the days of the conquest, she notes: "Iberian law codes identified women as 'imbecilitas sexus,' an imbecile by her very nature . . . women were classed with children, invalids and delinquents" (p. 141). This inferior status survives today, for example, in the Brazilian labor code which provides that " . . . husbands and fathers can bring action to terminate the employment of wives and daughters under 21 on the grounds of family disruption" (p. 219).

The "powers" enjoyed by women which Pescatello could have more fully elaborated include the widow's control of her deceased husband's property and the management of her impoverished household by the lower-class woman. We are briefly told of unusually powerful widows in indigenous tribes and in colonial families who assumed all of their husband's perquisites. We are also told that lower-class women have had more mobility outside the home, more personal freedom, and a kind of power their elite sisters were always denied because of the formers' economic responsibilities and the instability of their family life. But, at the same time, Pescatello concludes that "women who belong to groups on the periphery or totally outside the ken of power are as powerless as the groups to which they belong" (p. 232).

It is strange that Pescatello ignores the potential power of women within the economic arena. While she cites the nearly universal status of Iberian women as agricultural workers, domestic servants, and industrial laborers, she does not acknowledge either the centrality of this role or its importance for the whole economy. Mention is made of the black heritage of female participation: "[West African] female control over marketplace production and distribution has been a central feature of Afro-Latin, particularly Afro-Brazilian, cultures" (p. 89). For other areas of South America similar early involvement is noted: "In Colombia's Cauca Valley women dominated weaving enterprises and also totally controlled a labor pool of children under twelve . . . " (p. 122). In Argentina, Brazil, and other countries women and children are seen as the mainstays of nascent textile and food production industries. Unfortunately, though, Pescatello never focuses on

Iberian women as workers, a role which might have enhanced their power.

Pescatello proposes a new history. "We must," she says, "move away from the study of the narrow male elite groups and shift our attention to the heretofore inarticulate, the power *base* of history as it were rather than the power *manipulators*" (p. 233). She has not, in my opinion, advanced the cause of this new history with *Power and Pawn.* Certainly, she and other historians are handicapped by the very inarticulateness of the power base: if women have been illiterate and uneducated for centuries, they have been unable to transmit their own interpretation of historical events through normal channels. Even with such deficiencies, however, pieces of the historical picture that Pescatello tries to paint with a barn-sized roller can probably be studied in much greater depth than she has accomplished. Within the realm of social science, as a serious scholarly treatise, Pescatello's broad approach seems premature.

Pink Collar Workers: Inside the World of Women's Work. By Louise Kapp Howe. New York: G. P. Putnam's Sons, 1977. Pp. 301. $8.95.

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Ingrid, a waitress, is overworked and underpaid. Jackie and Linda, beauticians, and Peggy, a sales clerk in a large department store, are underpaid. Joyce and Bea, homemakers, are overworked but unpaid. Bonnie, an office worker, is underpaid—but her company provides free lunches. All of these women have male bosses, no unions (or else male-dominated unions), and few job benefits. With the exception of the homemakers, their work is so rigidly regulated they have little chance to express whatever creativity their tasks could allow. Their work, however, is not unskilled or undemanding. In this book, these women and others tell us about the ways occupational segregation affects their work lives. Regardless of their jobs, they have common complaints: economic exploitation, job insecurity, boredom, powerlessness, the impossibility of advancement, and the inhumanity of many of the (usually male) supervisors.

Pink Collar Workers is about some of the jobs traditionally filled by women. (Blue-collar jobs are filled primarily by men, pink-collar jobs by women.) Louise Kapp Howe focuses on only those jobs usually held by women without college degrees. These jobs are filled by approximately 60% of the paid female labor force, according to the Bureau of Labor Statistics (Bulletin 1880), and by the 60% of women who are not part of the paid labor force (i.e., homemakers). She does not consider women who work in "women's professions" of nursing, teaching, and social work or those who hold traditionally male jobs.

Howe does not attempt a thorough sociological analysis of occupational

segregation. Rather, she describes the results it has for women who work in pink-collar jobs through (presumably) verbatim interviews with women who work in five pink-collar occupations. These women articulate the problems and the possibilities of their jobs well, and because of them this is a very powerful book. Were it about past events rather than present realities, it would be called oral history at its best. In addition, the book is not devoid of sociological analysis. In her introductory and concluding chapters, through judicious editing, and in a running commentary, Howe explains why these occupations are structured as they are, who profits, and how the women lose. This analysis shows the ways in which the structures of the occupations are oppressive as well as the ways in which women contribute to (or at least accept) their own oppression. She treads delicately between blaming the men in power and blaming the system itself, but she does not adequately explain why the women do not, cannot, and, until very recently, have not attempted to change things. Her informants do, however, perhaps unwittingly, tell us why such attempts will meet resistance from women themselves as well as from those in power. As such, the book should make committed feminists despair. While it shows vividly why changes are needed, it also demonstrates the power that socialization and social institutions have in causing women to accept their own exploitation.

The major flaw in the book is its anti-intellectual bias. Howe has addressed the book to women without college degrees who are the most likely to work in pink-collar jobs, and she clearly feels that these women suffer more than women in other female jobs or in traditionally male jobs. She does not consider the possibility that women in all jobs share many of the problems of women in pink-collar jobs, whether they are in female or male occupations. But this bias also prevents her from identifying what is perhaps the most exploitive result of occupational segregation for women, and that is the degree to which women's educations are underutilized. Howe ignores the dual facts that education does less to improve wages or working conditions for women than it does for men and that many of the women she interviewed were capable of doing work which could have been more satisfying as well as more rewarding.

Lack of attention to this aspect of occupational segregation makes both the description and the analysis incomplete. However, the book cannot be ignored, because it fills an important gap in the literature on women and work. Lower-status women's occupations have been ignored by sociologists even more than lower-status men's occupations have. This book paints a vivid portrait of women's work life in pink-collar occupations and must be part of any consideration of women in the labor force or of the sociology of work.

Sex Roles: Biological, Psychological and Social Foundations. By Shirley Weitz. New York: Oxford University Press, 1977. Pp. xi+283. $10.00 (cloth); $5.00 (paper).

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Shirley Weitz has written a useful text for undergraduates studying sex roles. Her particular emphasis is on examining how a variety of biological, psychological, and social processes tend to maintain the "sex role system." Sociologists may find her discussion of the biological and psychological considerations interesting and informative but are likely to deem her treatment of social and cultural questions inadequate.

Weitz organizes her analysis by examining what she terms "The Biological Maintenance System"; "The Psychological Maintenance System"; "The Social Maintenance System: The Family"; "The Social Maintenance System: Symbolism"; and "Sex Role Change through Space and Time." For each of these topics, Weitz considers some of the key issues. For example, in considering the Biological Maintenance System she examines aggression, sexuality, and psychosexual abnormalities for evidence pertinent to sex-role differences. She is more successful when she uses this approach in the first two sections of the book, where she provides a neat, competent review of the relevant findings in biology and psychology. An omission of some importance, however, is the failure to address the work of the sociobiologists. The latter three chapters, on social and cultural considerations, do not come up to the same high standards of the first chapters.

A weakness in her conceptual approach is that, although the discussion in this text is organized around maintenance systems, these systems are never defined and seem to be used merely as chapter headings. The term "system" is also used with reference to biology, psychology, and sociology, as well as for sex roles. It would be helpful analytically if these concepts were carefully delineated. Particularly with reference to maintenance systems, the discussion might profit by demonstrating its theoretical linkages to Parsons's hierarchy of systems. Such an exploration could provide a powerful vehicle for discussing the interpenetrations of these maintenance systems.

Another serious problem is the limited questioning of the status quo. This is particularly puzzling when Weitz is concerned with producing change (p. 5). For example, why is the problem of aggressiveness not considered to be a social problem rather than merely a problem in males? And why is the menstrual cycle, which is perceived by some as disqualifying a woman for high public office, not contrasted with aggressiveness, which would seem a potentially more serious disability?

On another occasion Weitz states that "the differing consequences of sexual activity for men and women, short-term pleasure for one, possible pregnancy and long-term maternal responsibility for the other, have doubt-

less largely determined the nature of societal restrictions and structures" (p. 29). Maternal responsibility determines societal restrictions for Weitz. She does not consider that societal restrictions may determine maternal responsibilities. Evidence for such an interpretation is seen in the continual lengthening of the dependent period of children in our society. Moreover, sexual activity for women includes not only possible pregnancy but also pleasure and the power of giving life. In addition, monogamy and more specifically patriarchy might be conceived as capturing the power of life giving, that is, males possess the power by possessing the females.

A further illustration of this emphasis on the status quo is given when Weitz writes that "traditional attitudes linking men with the public sphere and women with the private, domestic one seem to underlie much of the resistance to sex role change" (p. 230). There is no attempt to explain the reasons for the maintenance of these attitudes, such as their service in restricting the competition for scarce resources in education and in professional careers.

Continuing with a consideration of Weitz's approach in examining the critical issues, the chapter on symbolism would benefit from additional attention to the arts, namely, literature, film, and theater. Television and advertising are other areas reflecting and reinforcing sex roles and thus also require investigation. The last chapter, entitled "Sex Role Change through Time and Space," takes us backward but not forward. For some reason Weitz avoids giving us a reason for the persistence of sex-role differences or an indication of what is probable in the future. A petty annoyance which is indicative of her lack of sensitivity to the use of sociological materials is Weitz's saying that she will examine sociological data and then introducing an anthropological study. On other occasions, she mentions a sociological system when what she may mean is a social system.

Minor disagreements with Weitz in terms of interpretation include her description of Jan Morris as having a "distaste for masculine activities" (p. 52). Au contraire! She seems to idolize men and their activities and to pity women. Also, with reference to Louisa May Alcott's *Little Women,* Weitz cites this classic as reaffirming "values of feminine domesticity" (p. 88). Perhaps, but we were also presented with a tomboy role model in the form of Jo, an individual who had intellectual aspirations.

Despite these limitations, *Sex Roles* may prove to be a valuable text for use in undergraduate courses. Most of the biological and psychological issues are discussed thoroughly, and, even though the treatment of social and cultural factors is more superficial, the material is likely to raise engaging and stimulating questions in the context of class discussion—which may have been Weitz's intent. We still need such texts in the area of sex roles.

The Black Male in America: Perspectives on His Status in Contemporary Society. Edited by Doris Y. Wilkinson and Ronald L. Taylor. Chicago: Nelson-Hall, 1977. Pp. viii+375. $17.50 (cloth); $6.95 (paper).

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Wilkinson and Taylor sought to "focus upon the social positions and role enactments of African American males" (p. viii) in contemporary American society. What results is a publication of considerable significance and importance to scholars concerned with the study of the black experience in this country. Drawing upon the work of sociologists, social psychologists, psychiatrists, and anthropologists, the editors manage to examine from an interdisciplinary perspective many of the myths and realities surrounding black men. Among the issues addressed in this unique volume are: the roles of black males as fathers, husbands, and providers in their families; male socialization processes in black communities; the form, functioning, and consequences of female-headed households for black males of all ages; the nature of societal barriers which restrict full economic participation by black males; black male–black female interpersonal relationships; interracial mating by black males and sex-role orientations in the black community. The book is organized into four sections: (1) "Socialization to the Black Male Role," (2) "Stigmatization: Systematic Branding," (3) "The Issue of Interracial Mating," and (4) "Roles and Statuses in Post-Industrial Society." Underlying and integrating these sections is a common concern with the social statuses and roles assigned to black males by this society. Through a masterful blending of original papers and selected works that have appeared elsewhere, Wilkinson and Taylor bring to bear on the black male condition in this country an enlightened, perceptive analysis quite unlike any I have seen before. When one considers the paucity of quality research which has been done on black males to date, this is no small accomplishment.

In order to convey better the substance of this timely book, I turn now to a brief summarization of its contents. Section 1 begins its consideration of black male socialization experiences with David Schulz's article, "Coming Up as a Boy in the Ghetto." Schultz's conclusion (reflective of what Ronald Taylor calls the "orthodox interpretation of the nature of black male socialization in matrifocal families" [p 4]) that black youth in this setting are socialized into ineffectual, deviant male roles is systematically questioned by the articles which follow. Ulf Hannerz points out in "Growing Up Male" that fatherless households are rarely devoid of male role models; by the same token mothers frequently instruct boys in so-called manly behavior. Charles Kiel develops the cultural aspects of the ghetto male role more thoroughly in his article, "The Expressive Black Male Role: The Bluesman." Robert Coles's examination, "Black Fathers," reveals the often ignored majority of poor black fathers who struggle against the severest odds in their efforts to care for their families. Quite distinct

from the orthodox view, these men are shown to be strong, proud, loving —and loved—figures in their families. Finally, Seymour Parker and Robert Kleiner's study, "Social and Psychological Dimensions of the Family Role Performance of the Negro Male," identifies societal discrimination in employment, housing, and general opportunities rather than subcultural norms as the major determinants of black male family role performance.

William Turner's "Myths and Stereotypes: The African Man in America" opens the second section with an in-depth analysis of the historical bases of myths and stereotypes about black men. In the process he offers a revealing critique of scientific modeling as it has been applied to the study of black males. Robert Staples systematically dissects and refutes two of the many stereotypes discussed by Turner in his articles entitled "The Myth of the Impotent Black Male" and "The Myth of the Black Matriarchy." It is Staples's conclusion that although both concepts are common to the literature, they lack legitimate bases in either theory or fact. Similarly, Harry Edwards lays to rest notions of black biophysical superiority—or inferiority, depending on one's perspective—in his article, "The Sources of the Black Athlete's Superiority." He finds black "superiority" in this arena to stem from the denial of equal opportunities to blacks in other fields of endeavor. Last, Doris Wilkinson's "The Stigmatization Process: The Politicization of the Black Male's Identity" offers a thoughtful consideration of color stigmatization processes and their detrimental consequences for the status of black males.

Beth Day's article, "The Hidden Fear," introduces the touchy topic of interracial mating by probing the hidden fears associated with black male–white female mating in our society. James Blackwell's "Social and Legal Dimensions of Interracial Liaisons" examines institutional barriers to increased interracial mating and predicts that as these barriers diminish rates of black-white dating and marriage will rise. Beth Downs supports such a view in her "Black/White Dating" when she links the dramatic growth in transracial dating and marriage to political liberalization of the society during the sixties. Selections by Calvin Herton ("The Negro Male") and Doris Wilkinson ("Expectations and Salience in White Female–African Male Self-Other Definitions") serve, however, as reminders of the continuing negative stereotypes and degrading stigmas associated with such unions. The taboo status of white females, mythical portrayals of black male supersexuality, and the white female's cultural right to establish the rules of black male–white female mating games are all examined in this context.

The Black Male in America concludes with an examination of contemporary and projected future statuses of black males in American society. Ronald Taylor's insightful opening paper assesses the causes and consequences of the continuing concentration of black workers in the lower reaches of the occupational hierarchy—a condition he fully expects will worsen over the coming years. He also expresses related concern over the "deepening schism" in the black community as middle- and upper-class salaries more rapidly exceed those of the much larger black underclass.

Nathan Caplan, in "The New Ghetto Man: A Review of Recent Empirical Studies," speaks to some political consequences of continued deterioration in the overall economic status of blacks and concludes that violent disruption of urban centers is a constant possibility. In keeping with the book's iconoclastic bent, Edwin Harwood and Claire Hodge's "Jobs and the Negro Family: A Reappraisal" convincingly challenges the myth of black female economic superiority over black males. Black females are shown to be the most exploited of all the economically deprived. Charles Willie's "The Black Family and Social Class" and David Schulz's "Variations in the Father Role in Complete Families in the Negro Lower Class" comment on the often neglected diversity of black families. These two papers show that family structural and interactional patterns vary across as well as within classes.

As the summary above suggests, this book is worthy of careful attention. If I were to criticize it, it would be for whetting my appetite and then leaving me unsatiated. After reading the book a host of questions were provoked in my mind concerning the status and roles of black men in this country's criminal justice, religious, military, and political systems. What also of the relative value of competing theoretical perspectives for understanding black males? Or, which alternative research designs should we strive to apply in the future? Are there lessons of importance to be shared with legislators and other policymakers whose decisions impact upon black males? The questions stimulated but left unanswered by this volume are endless. But then, this is perhaps as it should be. Wilkinson and Taylor have assisted in organizing our thinking about topics of relevance to the study of black males and have provided heuristic motivation for future research in the area. For this significant contribution they are, I think, to be congratulated. It now remains for those of us with interests in this area to respond to this impetus by more extensively researching black male statuses and roles in our society.

Turkish Workers in Europe, 1960–1975: A Socio-Economic Reappraisal. By Nermin Abadan-Unat and contributors. Leiden: E. J. Brill, 1976. Pp. viii+424. Fl 160.

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Over the past decade or so, the principal focus of studies in labor migration has been on three major geographical areas: (1) migration from Mexico to the Southwestern United States, which as farm labor has a strong seasonal character; (2) migrant labor in Southern and Eastern Africa, which is marked by racially defined wage differentials and "labor aristocracies"; and (3) current West European labor migration. In his paper "The Func-

tions and Reproduction of Migrant Labor" (*AJS* 81 [March]: 1050–87), Michael Burawoy has presented an interesting comparative analysis of the African and American cases, and S. Castles and G. Kosack (*Immigrant Workers and Class Structure in Western Europe* [London: Oxford University Press, 1973]) have dealt comprehensively with the situation in Western Europe.

One of the distinctive characteristics of the European case is the national divisions within the foreign work force. In West Germany in particular it had originally been a policy to recruit roughly equal shares of foreign workers from the various nationalities—Italy as an EEC member excepted—and to disperse these groups evenly throughout the country. Most work places and most company-owned housing reflect this dispersal. Commonly, two or three foreign nationalities are heavily represented, with sprinkles of the remaining groups. However, this balanced recruitment has been upset by the massive influx of Turkish and Yugoslav workers beginning in the late 1960s. Clearly, an attempt was made not only to separate native workers from foreigners, but also to stratify the foreign labor force internally, with the Turkish workers at the bottom of the wage scale and work hierarchy, and to pit one group against the other. It is interesting to note that this policy, aimed at preventing working-class solidarity, has not worked very well: foreign workers have supported most labor disputes and have often themselves initiated strikes, as in the case of the so-called *Türkenstreik* at Ford in Cologne in 1973, which was brutally suppressed by the police with help from their friends, the German labor unions. Unfortunately, not a single contribution in the present volume addresses this entire complex of questions.

A second important characteristic of the West European case is the type of its reproduction of foreign labor. If we use Burawoy's distinction of renewal and maintenance as the basic elements of reproduction, we see that there is a significant degree of variation among the different countries. Switzerland, at one extreme, separates processes of maintenance and renewal of labor most sharply: with an extraordinarily high ratio of foreigners in its labor force (roughly 30%, five times that of the other major European countries), it maintains a high degree of labor rotation, makes it virtually impossible for foreign workers to bring their families, and discourages vocational training and other forms of education. In this sense, it reminds one most clearly of the case of the South African mine workers. In complete contrast stand Belgium and Sweden, which tend to favor absorption of the immigrant into the new environment. By and large, however, Germany and France, with the bulk of the total foreign labor force, appear to be most representative of the European case: part of the foreign labor force is rotated, the other part shows a high degree of permanency, and a—very limited—attempt is being made to provide schooling or to offer vocational training. It is important to see therefore that, unlike in "pure" cases of migrant labor, in Western Europe some functions of renewal of labor are being shifted from the sending country to the receiving country.

Originally the influx of foreign labor at large helped to depress the wage level; today this task is assigned to the rotation segment (and the mass of illegal immigrants) in most EEC countries. It appears that this function of providing a rotating labor force was given primarily to Yugoslavia and Turkey. Several contributions by both German (Otto Neuloh) and Turkish (Turhan Oğuzkan) authors in the present volume directly advocate rotation even today, while little attention—except, in part, the paper by Ger Mik and Nia Verkoren-Hamelaar on foreign worker segregation in two Dutch cities—is given to the realities of relatively stable ghetto-type settlement, particularly of Turkish workers, in the receiving countries.

The case of European labor migration is further peculiar in that it forces an analysis of the internal character and the functions of the sending countries—a point often neglected in other regions of migration. This is perhaps the main focus of *Turkish Workers in Europe, 1960–1975*. Before the onset of its labor export, Turkey, like most other sending countries, underwent a sharp internal polarization between urban centers and the rural periphery. Unfortunately, Abadan-Unat's collection does not address itself to the causes of this polarization—for example, the agricultural policies of successive Turkish governments. Most of the contributors are content with the explanation that overpopulation as "push" factor just happened to meet the "pull" factors in the core countries of the EEC. Only Mübeccel Kiray, in a paper on the migrant worker's family structure, points briefly to the increasing indebtedness of the peasant as a cause of emigration.

While the conditions leading to emigration are not dealt with in a serious manner, several contributors point to the more visible concomitants of Turkey's internal polarization, especially the rural-urban migration within the country itself. This movement began roughly at the end of World War II and prepared the way for the international migration. Many of the migrants first moved from the rural to the urban areas and then moved on to central Europe. Furthermore, as Ruşen Keleş points out, roughly 50% of all foreign workers who left rural regions and went directly to Europe do not return to their hometowns later but settle in the larger cities. This process may be particularly pronounced in Turkey; it can also be observed in other sending countries such as Italy, Spain, and Greece.

Whereas the role of the sending country is to mobilize and export cheap labor to be consumed by EEC capitalism, it is the role of the migrant worker to open up markets, particularly by means of remittances, for the import of the commodities which they themselves help produce. Although foreign workers are underpaid compared with native workers, their high amount of overtime work, tax-free moonlighting, and their frugality, all of which can be sustained for only a limited time period, allow them to accumulate savings. In contrast to what appears to occur in the case of the South African mine workers, however, these savings are only partly used for immediate renewal functions—family subsistence—at home, and mostly for the purchase of consumer goods or unproductive investments back home,

as Abadan-Unat convincingly points out. While remittances temporarily improve the balance of payments of the sending country, they in fact help to increase the dependency of the sending country on West European capital. Some of this is dealt with reasonably satisfactorily in *Turkish Workers in Europe,* most notably in Günter Schiller's intelligent contribution.

In sum, this is a rather disappointing survey of the Turkish foreign labor question. The bulk of the contributions, often repetitive in substance, is replete with social policy clichés and well-intentioned banality ("The long-term goal must be to put an end to economically forced manopower [*sic*] movement" [p. 188]). Going through the volume, I have often wondered whether most of the contributors have ever ventured outside academic walls and actually talked to a migrant worker. Particularly appalling is the sloppy paper by O. Neuloh, who finds that people in underdeveloped countries do not like to engage in "handicraft and heavy industrial work" (pp. 53–54) and prefer office jobs, or that some aspects of Turkish backwardness must be attributed to its "national religious and biological mentality" (p. 72). Only M. Kiray presents a truly insightful study which points to the radical readjustment of migrants' kinship structures and to the drastic change in the role and the social status of women in migrant workers' families.

Finally, a word about the miserable job of editing this high-priced volume. I believe I have never seen anything in print so riddled with typographical and other errors. To mention just one example: "The labour market in industrial society will be determinated [*sic*] by the percentage of labour force [*sic*] depending in renumeration [*sic*] in wages and salaries" (p. 51). I hope this renowned international publisher is sufficiently embarrassed about his shoddy job.

Strangers in Their Midst: Small-Town Jews and Their Neighbors. By Peter I. Rose, with the assistance of Liv Olson Pertzoff. Merrick, N.Y.: Richwood Publishing Co., 1977. Pp. xiii+224. $10.95.

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As Marshall Sklare once noted, "Jews, more than any other group, have been among America's most enthusiastic city-dwellers" ("Jews, Ethnics, and the American City," in *Commentary* 53 [1972]: 70). Not only have they themselves adapted to urban life, they have also evolved a major role in supplying others with many of its amenities. Nevertheless, for most Jews such an urban existence was never a repetition of their European experience. There they lived in the small, rural village—the *shtetl.* Yet although many came from such villages, they seldom idealized that rural experience,

for they realized that in their *shtetl* existence they had occupied the bottom rung in the social order. The American city, on the other hand, symbolic of a new world where achievement rather than ascription was the determinant of social status and fortune, offered a new start. However difficult that start might be, most American Jews elected to be urban rather than rural in outlook and location.

In the late fifties, Peter Rose, then a graduate student in sociology at Cornell, decided to examine and interview those few Jews who, unlike 98% of their brethren in America, had chosen to live in small towns. He selected to study particularly those Jews living in the rural villages of upstate New York, the area most accessible to him. Twenty years later, one of Rose's students returned to the area, and to many of the families originally studied, for a brief follow-up. What emerges from this joint effort is a portrait of an American Jewish experience which was "swift as a shadow and short as any dream." The world of the small-town Jews of upstate New York is no longer; their children have gone to the cities.

At the time that Rose first talked to these rural Jews, their world was already in its decline, indeed seemed never to have really been on the rise. Unlike the rural world of the European *shtetl* which Mark Zborowski and Elizabeth Herzog poetically described as one where "life is with people," the world of the American village was for the Jew a "life without people." Again and again one reads that these people viewed themselves as "an isolated minority" and as "strangers." The rural Jew, living in a town with at most nine other Jewish families, missed the company of *his* people, and worried that his children might intermarry with Gentiles (although in the follow-up it turns out that few did). However successful economically and professionally the small-town Jew became, he saw himself relegated to the ascribed role of "ambassador," one who provides an acceptable model of his people for others. As Louis Wirth once put it, "getting out into the Christian world made the Jew self-conscious" (*The Ghetto* [1928; Chicago: University of Chicago Press, 1956], p. 123).

Yet instead of becoming "exemplary," conceived by their Gentile neighbors as representative of Jews in general, these Jews were viewed by those among whom they lived as "exempted"—different and special. Only occasionally did Rose discover evidence that Gentile attitudes toward the Jews were improved as a result of an intimate and positive experience with a particular Jew. Over 80% of the Christian opinion leaders whom Rose polled in his small towns continued to think of Jews in terms of the negative stereotypes of shrewdness, aggressiveness, clannishness, and the like.

Perhaps the particular questions that Rose asked emphasize the point, but the small-town Jew that emerges from these pages is one who constantly expresses—either directly or indirectly—a desire for legitimation by the Christian world. This Jew feels the urge to try to define his position vis-à-vis his non-Jewish environment. He explains his ritual and holy day observance, such as it is, in Christian terms. Passover becomes, for example, "a sort of Jewish Thanksgiving." He practices only those rituals which can

be made somewhat sensible in the American Christian context. He describes himself in terms which belie anti-Semitic, Christian stereotypes; Jews are "not underhanded or shrewd." In short, whether happy or not in the small town he inhabits, he acts as if he were there by the consent and sufferance of the dominant Gentile culture. Any ethnic pride he may have is muted in its presentation.

The lesson of this life was not lost upon his children. Most have left, not really to the surprise of their parents. Nearly half those polled by Rose expressed a desire to have their children leave the small town. That is, they realized that however adjusted they themselves may have become to rural American life, the American small town is not a place where the Jewish people may feel at home.

What had drawn them there had been economic needs. In the small town a peddler could more swiftly transform himself into a merchant, and many did. In the small town a refugee physician could more easily practice medicine, and many did. But the price for this economic and professional success was often an overwhelming sense of alienation. Even those who expressed a satisfaction with rural life felt the constant threat of being swallowed up by that Christian world that had allowed them entry. "Of all the problems the isolated Jew must face, none is more pressing than that of preventing his children from becoming *goyim,*" Rose tells us (p 151).

If one is to judge by Rose's sample, these Jews were at least successful in avoiding the apostasy of their progeny. Their children seem to have followed the pattern predicted by the historian Hansen: "what the son wishes to forget the grandson wishes to remember." They are aggressive and contra-acculturative in their Jewishness. Not only have many left the small town, they have begun in significant numbers to openly practice their Judaism and to seek publicly to identify themselves distinctively as Jews.

But, one wonders, is this the true story of small-town Jews? Rose's method of collecting his sample, snowballing (asking rural Jews to give names of other rural Jews they know), seems to assure that he would speak to those who remained bonded to the Jewish community, however tenuously. What of those Jews who left completely, who moved to the West and the South? What of the Goldwaters, and others like them, who so integrated themselves with rural America that they ceased to hold any identity as Jews? These people, even more than Jews in rural New York state, were the small-town Jews of America. These seem to forever have been lost.

Nevertheless, if Rose and Pertzoff have not told the entire story of the American small-town Jews in *Strangers in Their Midst,* they have given us a unique glimpse of one American Jewish character. Perhaps most interestingly, this description of a deviant Jewish life form is further evidence that in America, as in Europe, in the final analysis, "a Jew," as the philosopher Nathan Rotenstreich once put it, "is a Jew only when he is with other Jews" ("Emancipation and Its Aftermath," in *The Future of the Jewish Community in America,* ed. D. Sidorsky [New York: Basic, 1973], p. 56).

Community and Polity: The Organizational Dynamics of American Jewry. By Daniel J. Elazar. Philadelphia: Jewish Publication Society of America, 1976. Pp. xv+421. $12.00.

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Daniel Elazar belongs to that small minority of Jewish social scientists who have very strong Jewish commitments. He is distinguished by the fact that he has both an intimate knowledge of contemporary Jewish affairs and an acquaintanceship with classical Jewish sources. Elazar is a rare individual: a scholar and an activist, a general theorist as well as a political scientist passionately devoted to the furtherance of contemporary Jewish studies, an academic administrator as well as a scholar who is both a teacher and a researcher.

All of Elazar's diverse interests and qualities are evident in *Community and Polity.* The result is a book which is unique and important. The true significance of *Community and Polity* is apparent when it is compared with the only other book on the structure of the American Jewish community: Maurice J. Karpf's *Jewish Community Organization in the United States* (New York: Bloch). As Karpf's book was published in 1938, it contains no information on the events of the past four crucial decades. In addition, Karpf's book is limited by being written from a social work perspective—in fact, the impetus for the book was the Second International Conference on Jewish Social Work, which was held in London in the summer of 1936.

Elazar covers some of the same ground traversed by Karpf, but he does so in an entirely different way. Instead of systematically treating which organization delivers what social services, he analyzes the principles of organization on which the American Jewish community is based. Painting with broad strokes, Elazar excites interest by moving away from the social work approach which has characterized writing on the Jewish community. His book is novel because he writes about the Jewish community in the same way that political scientists write about community in general. With the exception of the works of Charles Liebman, this type of analysis has been sorely lacking in writing on the Jewish community.

Elazar's book covers a host of topics. He analyzes the degree of pluralism in American society and studies the volunteerism on which the American Jewish community is based. He analyzes the American synagogue in its diverse forms. He provides a great deal of information on the federations—the fund-raising agencies found in all cities with a Jewish population of any substantial size. The federations subsidize local services and act as a central address for the Jewish community. A substantial part of the receipts from their annual campaigns is allocated to national and international Jewish organizations and to the needs of Israel.

Elazar attempts to delineate the process of decision making in the federations and in the American Jewish community in general. He analyzes

what he calls the "trusteeship of givers and doers" (p. 336). Furthermore, he describes the uniqueness of the various regions of the nation and the special characteristics of Jewish communal life in the cities located in these regions. He also proposes a scheme for analyzing the relationship of a given individual to the community. Discerning seven levels of participation in communal life, he provides an estimate of the proportion of Jews on each level. Thus, Elazar has written a volume which will serve the purpose of introducing social scientists to the phenomenon of the organized Jewish community. *Community and Polity* will also be a book which no student of contemporary Jewish studies can afford to ignore. Finally, it will undoubtedly serve to raise the level of writing and discussion about Jewish communal affairs.

The defects of *Community and Polity* are two. The first is a combination of carelessness, incompleteness, and sloppiness. For example, although Elazar devotes several pages to a discussion of the Jewish community center as an institution in American Jewish life, his index does not contain the entries "community center, Jewish" or "Jewish community center." Elazar's footnotes are replete with errors. In a single sentence on page 406 there is a typographical error in the last name of one Jewish educator (Judah Pilch) and in the first name of another (Alvin Schiff). Appendix D, entitled "Jewish Community Studies: A Selected Bibliography," obviously requires a period of uninterrupted work in a library with a large Jewish collection to bring it up to standard. For example, in his section on Boston, Elazar fails to list the standard book in the field: *A Chronicle of Boston Jewry* (Boston, 1963) by Albert Ehrenfried. The section on Chicago has the same defect—Elazar fails to list the standard book: *A History of the Jews of Chicago* (Chicago: Jewish Historical Society of Illinois, 1924), edited by H. L. Meites. Elazar also takes delight in breaching confidentiality; his practice is to identify the true name of communities for which authors have constructed pseudonyms.

The second defect of *Community and Polity* is ideological and thus cannot be remedied so easily. Elazar's central view is that two institutional forms have emerged as predominant in the American Jewish community: the synagogue and the federation. Elazar is well acquainted with the American synagogue and is sympathetic to its purposes. He is, however, troubled by its localistic orientation and by its lack of articulation with the American Jewish polity as a whole. The end of the matter is that he views the federations as the center of the Jewish community. His picture of the federations is almost entirely positive. It is, in fact, out of keeping with what might be expected of a treatment of this institutional form by an academician.

Although the federations have been quite successful in raising large amounts of funds, it is apparent that they have avoided addressing basic problems of the Jewish community since most of these problems are not philanthropic in character. Federations raise large sums for Israel. But Israel's problems are political, and the crux of American Jewish responsi-

bility is the task of mobilizing political support for Israel. The problem of Soviet Jewry is also a political one. Although the funds raised by federations have been helpful to Soviet Jews who have settled in the United States and in Israel, the federations have been peripheral to the effort to fight restrictions on the right of emigration from the Soviet Union and to improve conditions for those Jews who wish to remain there. On the local scene the most obvious problem is that of decaying Jewish neighborhoods. Since this problem too is basically political, federations have either been passive or impotent. Whatever strategies have been utilized to stabilize Jewish neighborhoods have come from local residents and from neighborhood-based Jewish organizations rather than from the federations.

Elazar is surely aware of all of these problems, but they do not seem to give him pause. He does not address himself to whether the federations take on only the problems which they feel are manageable and leave to others the really difficult questions of Jewish life. Elazar also does not discuss whether the solution to whatever issues federations do take on is invariably the traditional Jewish response of *tzedakah,* or what may be loosely translated as philanthropy.

There can be no objection to an academic becoming an activist. It is refreshing to see the example of an Elazar who does not feel the alienation which academicians often experience when they work very closely with the "trusteeship of givers and doers." On the other hand, *Community and Polity*—with all its admitted excellences—leaves us with the question of whether the social scientist should not keep a certain distance between himself and the politics of the community lest he be attracted to the conventional wisdom.

World of Our Fathers: The Journey of the Eastern European Jews to America and the Life They Found and Made. By Irving Howe, with the assistance of Kenneth Libo. New York: Simon & Schuster, 1977. Pp. xx+714. $6.95 (paper).

Jack Nusan Porter
University of Lowell

This book (at times it seems better to call it an encyclopedia) is a Jewish *Roots,* just as *Roots* is a black *World of Our Fathers.* Both Alex Haley and Irving Howe have achieved what is probably the goal of nearly every sociologist—to write a serious sociohistorical narrative that becomes a nationwide best seller. Now, if only a sociologist could accomplish this, it would give our shopworn profession a shot in the arm!

Both Haley and Howe have combined primary scholarly material (early newspapers, shipping records, immigration and governmental reports, first-person memoirs) with a compelling novelistic writing style to produce a

book that is both a joy to read and a contribution to historical scholarship. Here I will only touch on a few sociological highlights.

Howe writes with supreme confidence and with a total grasp of his subject matter. He combines a knowledge of Yiddish literature and literary criticism with a knowledge of Jewish contributions to socialism and trade unionism. To be at home in both literature and politics is a rare and delightful combination. The book flows effortlessly from the persecution of *shtetl* Jewry in Eastern Europe, through their passage to America, their settlement on the Lower East Side of New York (which at the turn of the century had a population density that equaled the worst sections of Bombay), their struggle for success and dignity in the *goldineh medineh* ("golden land of opportunity"), their contribution to American culture, to the unease of the children and grandchildren, who still struggle to maintain their identity as Jews. It is the story not only of the Jews but of every immigrant group—and every immigrant group should find a Howe or a Haley to record its journey to America.

Howe's strongest chapters are those dealing with the Yiddish press, the socialist/trade unionist movements, and the role of Jewish scholars, intellectuals, and entertainers in American life. As a sociologist, however, I was irritated by his obvious blind spots regarding the role that sociologists (especially the Chicago School) played in such areas as social reform, immigration policies, and the study of immigrant pathology.

To be sure, he does mention some people briefly, for example, social historians such as Jacob Lestchinsky, Elias Tcherikower, Mark Wischnitzer, and Jacob Shatzky; essayists and philosophers such as Hayim Greenberg, Morris Raphael Cohen, and Hayim Zhitlovsky; Holocaust writers such as Isaiah Trunk, Philip Friedman, and Jacob Robinson; and Labor Zionist political philosopher Nachman Syrkin and the sociologist C. Bezalel Sherman. Most sociologists have probably never even heard of these people. Furthermore, many sociologists may also not know (as I myself did not) the answers to several questions that Howe brings up: What side did sociologists take on the immigration controversies? Did they favor "open admissions" or restrictive quotas? What were the prevailing political attitudes of sociologists toward reform, pathology, etc.? They studied immigrants, and quite well, but I am curious about their personal attitudes on the political issues of the day. Howe's book tantalizingly opens up such avenues of inquiry.

However, he makes no mention of the important contribution of the Chicago School to immigrant studies. Prominent members of the school were intrigued by the Jewish immigrant: W. I. Thomas (who is not mentioned) did a study of the "Bintel Brief" ("Bundle of Letters") column found to this day in the *Jewish Daily Forward,* the largest Yiddish paper in the world, and he learned Yiddish to make the study. Ernest Burgess, Ellsworth Faris, and Robert E. Park encouraged and guided Louis Wirth in writing the now classic *The Ghetto,* which is never mentioned by Howe. There is work for sociologists to do in this area where Howe barely treads.

He is very good in describing the "social dislocation and pathology" (p. 96) of the Jewish quarter on the East Side. He practically entices the reader to go back in history to verify or refute his generalizations. For example:

> There was crime, there was wife desertion, and there were juvenile delinquency, gangsterism, and prostitution during the eighties and nineties, as well as during the early decades of the twentieth century—probably more than the records show or memoirists tell. How could there not be?
>
> Precise information on these matters is hard to come by, and the reasons are obvious. Communities struggling for survival seldom rush to announce their failures. The craze for sociological investigation that would overtake America in the twentieth century was not yet very strong. And over the centuries the Jews had developed a cultural style encouraging prudishness and self-censorship: there were things everyone knew, had no choice but to know, yet only rarely was it deemed proper to speak or write about them. Life was hard enough without indulging in luxuries of revelation. [P. 96]

> The single greatest journalistic quality of the [*Jewish Daily*] *Forward* was the sustained curiosity it brought to the life of its own people. In a later, more portentous age, this would have been called "the sociological imagination." . . . Were East Siders finding new occupations ranging from real estate to gangsterdom? Were lonely immigrant girls succumbing to the lure of suicide? Were *yentes* (busybodies) moving to West End Avenue and becoming "fancy ladies"? . . . Nothing seemed too mundane for the *Forward* staff, and that may be one reason high-minded Yiddish critics often treated the paper with contempt. [P. 531]

It is interesting that Howe's strongest areas have come under the most attack from some Yiddish quarters, from people with long memories. Paul Novick, the editor of the *Morning Freiheit,* the oldest Jewish-Communist paper in the world, has recently written a booklet entitled "The Distorted 'World of our Fathers' " (*Morning Freiheit,* New York, 1976), wherein he condemns Howe in part for his errors regarding some of the early trade union strikes but moreover because he does not berate Abraham Cahan (the founding editor of the *Forward*) enough for vulgarizing Yiddish by introducing too many Englishisms into the language and for ridiculing "highbrow" Yiddish literature (e.g., Sholem Aleichem). The old struggles, unknown or forgotten by the children, are still remembered by the old-timers.

But, even if true, these are really minor criticisms for a book that tells so well the story of *Yiddishkeit* and *Yiddin* in the New World. I recommend it to every social scientist, and, now that it is out in paperback, it is affordable for students as an auxiliary textbook.

Ruling Class, Ruling Culture: Studies of Conflict, Power and Hegemony in Australian Life. By R. W. Connell. Cambridge: Cambridge University Press, 1977. Pp. xii+250. $19.95.

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R. W. Connell is certainly one of the most interesting and stimulating personalities in Australian sociology. His first significant work was a study (à la Piaget) of the early political socialization of children. By the mid-seventies he apparently converted to Marxism and discovered the Australian ruling class for the Australian academia. His short, well-written essay (theoretically not much beyond an elementary Marxism-Leninism seminar), "Yes, Virginia, There Is a Ruling Class" (with T. Irving, in *Australian Politics*, ed. H. Meyer and H. Nelson [Melbourne: Cheshire, 1973]), is still compulsory introductory reading for all would-be radical sociology students on Australian campuses, and with its publication Connell at once became the *enfant terrible* of the sociology establishment. Certainly in the seventies it would have been difficult to shock anybody in the United Kingdom or in the United States with the rediscovery of class analysis, but Australian sociology lagged behind the mainstream of research. With the exception of Sol Encel, whose *Equality and Authority* (Melbourne: Cheshire, 1970) is still the only comprehensive analysis of Australian social structure, few sociologists in Australia showed much interest in macrostructural questions. Thus Connell moved into a theoretical vacuum and, surrounded by admirers and enemies, attempted as a one-man task the almost impossible—to rewrite Australian sociology from the point of view of class analysis. Readers of *Ruling Class, Ruling Culture* should bear in mind the pioneering character of this work.

Ruling Class, Ruling Culture is a collection of essays written between 1972 and 1977. These essays are partially work-in-progress papers in which Connell attempts to understand the meaning of class and the differences between class and stratificational analysis, and partially writings for a more general audience on such wide-ranging topics as the short-lived rule of the Labor Party, the nature of the mass media, a 15-page sociology of Australian big companies, and a similarly condensed overview of the problems of class consciousness in childhood. These later essays were written with the obvious purpose of demonstrating to the wider public that the new sociological approach is justified and helps to explain the most varied phenomena. Professional sociologists, therefore, should not expect to learn much about the actual "conflicts, power and hegemony in Australian life" (although this is what the subtitle of the book promises). Rather, I think we should look upon this collection of essays as preparations for a more comprehensive work. These papers should help Connell to write and his readers to appreciate his still unfinished book on the class history of Australia.

In the first two essays, which I called work-in-progress papers above, Connell offers a criticism of the stratification approach to social structure. There is nothing spectacular or original in this critique (it is not very easy to be spectacularly original in such an area so much worked upon), but Connell's distinction between class structure and stratification is clear and precise and does not lack a certain degree of sophistication: he identifies class theory as a "generative" one, stratificational theory as a "categorical" one. One only wonders why an author, who is thus aware that we are dealing with analyses on different levels of abstration and with different purposes, engages himself in a full-scale attack against stratification research. Connell makes the same mistake as his stratificationalist opponents who wanted to replace class with stratification. Connell accepts this false either-or dilemma. Instead of explaining to us what a class analyst can do with data on social stratification, he rejects stratificational analysis altogether.

Connell's own understanding of class has undergone a significant change since "Yes, Virginia, There Is a Ruling Class." From the two first chapters one gets the impression that he still operates within the classic Marxist paradigm, but from the more empirical essays that follow it is clear that the author moves away from a class definition based on ownership and increasingly treats power as a class-generating factor; he moves from Marx toward Dahrendorf. The reader is not warned of this significant shift in theoretical orientation, and one has to be quite familiar with different class theories and their cobwebs to realize what the author is doing here. Connell tries to avoid a narrow definition of the ruling class and thus proposes "an extension of the initial definition of ownership and the concept of profit" (p. 49). However, he finally decides not to attempt this extension on the theoretical level but to look empirically at the managerial elite, at the leadership of the Liberal and Country party, and at the top civil servants. He classifies them (partly by reverting to the logic of stratificational analysis) into the ruling class, basically on the grounds that they exercise power in the interest of capital. In an implicit way (for he does not attempt to interpret the notion of the working class) we are confronted with a class dichotomy between the rulers and the ruled, the powerful and the powerless, which seems quite a few steps away from Marxist class analysis.

One hardly can question the political relevance of this theoretical turn. In addition, Connell does not hesitate to mix a bit of psychoanalysis into his class theory if it is necessary in order to strengthen his argument. This is most obvious in the last essay, "Pattern of Hegemony," which may also be the most original in the book. Here Connell applies with a great deal of imagination Gramsci's concept of hegemony, although he probably overstates the deterministic overtones of this notion.

Connell's overtures to theoretical eclecticism, however, worry me. As far as I can judge, in this move he shares the growing pains of many left-wing social scientists in Australia for whom the theory is nothing, the practice is

everything. We should be aware that the subordination of theory to the needs of political practice is the end of theory.

Ruling Class, Ruling Culture is not a great book, but it is a stimulating one and I can warmly recommend it even for use in undergraduate teaching here. We are looking forward to Connell's next book on Australian class history, which I expect will show more theoretical rigor and be based on more solid empirical evidence.